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ABSTRACT

This document is a transcript of the proceedings of a one-day meeting held in Austin, Texas on March 29, 1967. Participants included representatives of the national Head Start Planning Committee, the Head Start Research and Evaluation Division in Washington, D. C., and the staff and study directors of the regional Child Development Evaluation and Research Center at the University of Texas at Austin. Discussions concerned the initiation of Project Head Start research in the Southwest, the scope of Head Start evaluation, evaluation study samples and statistical strategies, and research reports on: (1) the effect of neural conductivity efficiency on cognitive development, (2) the relationship between dialect and socialization, (3) the effect of response style on perceptual skills, (4) the oral language development of Head Start graduates, (5) the development of tests for bilingual children, (6) cognitive style and conceptual conflict in culturally deprived children, (7) predicting learning disorders from early life history, and (8) influencing parent and teacher attitudes by rewarding children. (Author/BRT)

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PROCEEDINGS

of a

MEETING OF THE HEAD START PLANNING COMMITTEE

with the staff of

THE CHILD DEVELOPMENT EVALUATION-RESEARCH CENTER

(Southwest)

THE UNIVERSITY OF TEXAS

at

AUSTIN, TEXAS 78701

March 29, 1967

PS 008563

Foreword

This document presents the moment-by-moment Proceedings of a day-long (March 29, 1967) meeting held in Austin, Texas, between representatives of (1) the national Head Start Planning Committee, (2) the Head Start Research and Evaluation Division in Washington, D. C., and (3) the staff and study directors of the regional Child Development Evaluation and Research Center, supported by Head Start, at The University of Texas at Austin. In some places these Proceedings have been edited for one or more of three reasons: (1) to straighten out the language misusages which often occur in lengthy oral exchanges; (2) to remove unnecessary redundancy and prolixity; (3) to improve the continuity of the document by deleting extraneous or seemingly garbled remarks. Such editing has not, I believe, altered the substance of the Proceedings in any important way. Nonetheless, the original set of the Proceedings, accompanied by the Stenotypist's notarized certificate asserting that the copy is a true and complete transcription is retained in the files of the Child Development Evaluation and Research Center at The University of Texas at Austin.

John Pierce-Jones

Austin, Texas
March 29, 1967

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HEAD START PLANNING COMMITTEE
MEETING WITH THE
CHILD DEVELOPMENT EVALUATION AND RESEARCH CENTER (SOUTHWEST)

March 29, 1967

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OPENING REMARKS

John Pierce-Jones

The University of Texas

I understand there are eight people from the Research and Evaluation Division of Head Start in Washington and the Planning Committee. This is about fifty per cent representation, and I suspect it is par for the course.

We have prepared an agenda that covers the full day. I don't think you can see what we are doing unless we take that much time. And we need to run it through at a pretty rapid clip.

We are going to ask you, at least in the beginning phases of our discussion, to name yourselves for the benefit of our Stenotypist, who is going to make a record of today's proceedings.

I thought it would be nice if you had the opportunity to meet the Dean of our College of Education, Dr. Wayne H. Holtzman, who is also Professor of Psychology. This is an interesting and unique combination of roles in American Universities.

WELCOMING STATEMENT

Wayne H. Holtzman

Dean, College of Education

Thank you, John. It's a pleasure to be with you this morning. This is an area (research on compensatory education) that is very close to my own interest, and I thought I might take this opportunity to spend just a couple of minutes telling you about some of the changes going on in the College of Education in The University of Texas and in this region of the country. These changes are rather typical ones, they are the kinds of changes that are going on in universities all over the country. Education has really come alive in the last couple of years. Most of you are bouncing around, as I am, and know how alive it really is.

The University of Texas is probably the only large, major, national university in the region. It covers a very wide radius, running out to the West, clear over to the West Coast and up to the Midwest. This means that we have a considerable responsibility not only for regional and State development but for certain national kinds of development.

The enrollment picture at the graduate level has been almost doubling every year now for three years, and the problem this is

creating for the University is becoming very acute. As you know, graduate study and research requires considerably more investment in space, resources, and in personnel than does undergraduate work. In our case, where we have a doubling of student enrollment, we may have no more than a twenty-five or thirty per cent increase in facilities. The faculty are increasing at over a hundred professorial appointments a year. This has been going on now for four years.

This rate of absorption is equivalent to several new buildings a year - buildings which are not yet being built. I say this partly to emphasize the acute problem of space which goes along with the tremendous growth. Over a five to ten year period, this will level out and take care of itself, but presently where you have these sharp accelerations, the available space lags behind. This is one of the reasons why this particular Center is located several blocks off campus rather than on campus; in rented space, rather than in what you would call prime academic space. We have had to establish emergency conditions on the university where the prime academic space is reserved for the main enterprise of the University, the teaching of the undergraduate and graduate students, and the conduct of research activities associated closely with this. So, most of the organized research activities have been put on the edge of the University until we can plead our building program over the next five years.

The College of Education will be getting about a seven million dollar complex which will be about a five-fold expansion in space over the next several years. We have had an entire campus area designated for the College and this will have associated with it research facilities of the kind you will be looking at today only they will be integrated into the total academic plant at that point.

Going back into the context of the Research Program in the Southwest Child Development Evaluation and Research Center, I might mention the long standing interest at this University in longitudinal studies, in studies of cognitive development, personality and social development in children. A center of this sort offers a wonderful opportunity on the regional level for development and from a basic scientific point of view. It can provide the "base line" work for longitudinal studies over the coming years. I am personally involved in longitudinal research under the National Institute of Mental Health. This is the sixth year of a seven year project involving repeated testing of four hundred and fifty school children in Austin and a comparable number in Mexico City. A number of experimental tests are being used and we are looking at the environmental pressures in the home through study of the parents and family configuration and at what is going on in the school and how this relates to child development. So that with my personal investment in it, you can see why I am particularly pleased to have a chance to talk to you for just a few minutes.

Let me shift now to something quite different. In meetings that I have attended at the National Research Council, and in recent ones of a special committee to survey the behavioral sciences set up by SSRC and NRC, we have had discussions about what are the "hottest" areas in the behavioral sciences, particularly in psychology. One of them that comes up repeatedly is the matter of "early stimulation" and of its impact on later maturation and learning. I think the Life Magazine article that came out several days ago typifies the public interest in it. And to a man around the committee, this was viewed as one of the prime areas to present in the committee's report surveying the behavioral sciences, simply because of its significance to our society.

For any of you who are writing scientific articles growing out of research in early child development, as the new editor of the Journal of Educational Psychology, we -- I, as the editor, and my editorial board - are trying to emphasize developmental psychology within educational psychology. And at the moment there is no Journal of Developmental Psychology under the American Psychological Association. It looks like the child-oriented rather than animal oriented research in developmental psychology is likely to emerge within the Journal of Educational Psychology. So if any of you, yourselves, or your colleagues, have any material coming along,

particularly out of research of this kind, which you feel is of interest to scientific audiences, I would appreciate your sending it to me so that we can consider it for the Journal.

Discussion

MR. SUGARMAN: If I might make one observation. In terms of longitudinal research--there are now, in developmental states, two additions to the Head Start Program. One goes to younger ages, and the other goes on into the school system. In fact, then, we have a real need for a longitudinal study that starts almost with birth. And we can follow up through the entire school system. And we would value ideas on this.

DR. HOLTZMAN: I am certainly glad to hear that. I think that out of the various centers which have skilled examiners, we have been doing a careful job of sample selection and testing. Now we have, really for the first time, a large sample with cultural diversities located throughout the country, e.g., different ethnic groups. It's possible for the first time to study these children in large enough numbers to hold constant, statistically, the variance which is confounded with some of the things we are interested in. Over the next three to five years, longitudinal work growing out of this base line data would be a very good investment.

MR. SUGARMAN: Secondly, there is a question. You have indicated that Texas has the major state university in this area. I'd like to know, generally, what you consider to be the area of influence of Texas. I ask this question because we have problems in certain of your surrounding states in getting these people interested.

DR. HOLTZMAN: This is a good question. You might ask whether the University of Texas is a southern university or western university. It has only nominal ties with the South through SREB. Most SREB activities are centered in Atlanta and radiate from there. So, at the moment, I can't name any SREB activities we are involved in.

If you look at the superficial characteristics of The University of Texas - 1,700 faculty, about 5,000 graduate students, 28,000 total full time students - and the various programs in which it engages, you will find that it has as its competitors, if you look to the west, UCLA and Berkeley. Looking to the north, you go up to Iowa, Illinois, Indiana, and the big ten universities. If you look to the east, you go to North Carolina and Duke. To some extent, Florida and Florida State are beginning to rise, but they have a long way to go before they have the critical mass that The University of Texas has at the moment. We are fairly comparable to the University of Wisconsin in that we have a tremendous variety of programs and we are in a small city, a state capitol. We are different from any of

the other universities that I have named in that you have to go eight hundred miles, a radius of eight hundred miles, to find any other like it. So we do draw on a very large region in this center. We used to be a state university of the more provincial sort. In the last ten years, that's changed very dramatically.

DR. ZIGLER: I wonder if I might ask a question, of my mentor. It's not often that we have such high class brains that we can pick -- those of us genuinely committed to the research enterprise of Head Start. We have had a continuing problem, if problem is the right word, on the research committee, on which I sit as a kind of member, and that is "Where do we want to bet our money?" I think it's a real tribute to Head Start that it has committed itself to the research enterprise; it's been shaky from time to time, but I think we are making progress. I would really like to hear your opinion about the longitudinal study, in this respect, Dean Holtzman. We have set up these regional centers of which the operation here in Texas is an outstandingly good example. And so we now have the instrumentality to do the kind of longitudinal study that Jule Sugarman mentioned to you. This, of course, would necessarily involve competent investigators throughout the country.

At our research meetings, there have been some complaints about such an effort, although I tend to think it would be a very valuable one and have thought so from the beginning. But the argument is made that more could be learned through autonomizing.

DR. HOLTZMAN: Giving them (E & R Centers) independence?

DR. ZIGLER: Right. And letting them engage in a variety of research, activities of a more experimental sort, rather than the constant evaluation that would be involved in longitudinal studies of the kind that Jule Sugarman mentioned. If you were going to spend the money, what would be your preference in this matter?

DR. HOLTZMAN: I would try to set up a situation to preserve some of both. First of all, I think it's reasonable to expect any Center that's cooperating in basic research related to an action program, if I can make that distinction, to give a bit in return for what they may be personally interested in having supported. I see no reason why you cannot demand a certain "core constancy." Now, what the size and scope of the "core" should be is one problem. If it's decided that the Stanford-Binet Scale is one you want to use as a base line measure, everyone should give it in exactly the same way, allowing, of course, for a certain amount of semantic and other kinds of translation in order to give semantic equivalents,

for the examination. That is, if you are testing Spanish-speaking children you have to give the test a little differently than if you are testing English-speaking children, Indians and others. The aim should be to take a core of measures of variables that are crucial for national development and for understanding child development and make certain that they are as constant as possible. Given that core, there ought to be a "reserve for freedom" to extend beyond that as long as you don't destroy the meaning of the core.

This is the way in which I relate to my counterparts in Mexico City in my own longitudinal study. We have a core of techniques that we use repeatedly each year within two weeks of the birthday of each child in whom we are interested. And we spent several years training the observers and examiners who were working in Spanish instead of English. Careful translation of techniques is vital; you can't translate the Stanford-Binet or the Wechsler literally. You have to develop some new items or a test won't work across ethnolinguistic groups. At any rate, we are very pleased with our core - everyone, to a man, who is involved in this crosscultural research.

In addition, we have some reserve for quite a bit of experimentation. In a given year, we may phase something new into our program - something that grows out of this experimentation.

The second criterion, that I think is crucial in longitudinal research, is the stability and assurance of funding beyond one year. I know that with federal funds, you can't give more than an expression of intent, because monies have to be appropriated all the time. But efforts need to be made to solve this diversified problem so that highest priority is given to a commitment to longitudinal research. If you have to support research only for a year at a time, everyone is going to hedge. They are going to make sure they have something salvageable at the end of twelve months, and this might not be desirable from a longitudinal standpoint.

DR. PIERCE-JONES: Dr. Holtzman has been really quite supportive of our work and I wanted you to have the opportunity to hear his perceptions of things.

INITIATING HEAD START RESEARCH IN THE SOUTHWEST

John Pierce-Jones

The University of Texas

You have a program in front of you, and I am going to try to do some time makeup here. I am going to say very little about the concept of our work except that in June of 1965, I submitted a proposal to Dr. Gordon for an evaluation, Texas-wide, of the summer '65 Head Start Program. Admittedly, we met with all kinds of adversities and emergency situations, but we tried to solve them. I think we have been able to demonstrate two or three things of more than momentary interest.

One item that hit me most clearly, was that we seemed to get relatively clear evidence that you can produce statistically significant cognitive changes in children as measured by such tests as the Caldwell Preschool Inventory and the Peabody Picture Vocabulary Test, and this in a relatively short period of time. We found the "break point" to be about two weeks. Under two weeks, we didn't get significant changes; over that, we did in analysis of variance experiments.

Contrary to our hypothesis, we did not find that the direct observational measures we had made of teacher's classroom behavior contributed nearly so much to the predictions of changes in children as I had hoped - not nearly so much, certainly, as our knowledge of the youngsters' initial status on measures of vocabulary, academic skills, and so on.

Nonetheless, in some instances, we did find that observably measurable differences among teachers in their classrooms affected the prediction of changes in the children under their care. We developed during that program an instrument, a rating scales instrument, the Observers Rating Form (ORF) which has become the focus of a lot of controversy, which we hope will be resolved over the next year or two.

To pick one more point from what was really a very long set of studies, let me note that we had no control group in our summer evaluation experiment. The question was, then, how do we somehow get around this problem? Some eight weeks after the Head Start children had entered the first grade, we went out to their

first-grade classrooms, those where teachers had both Head Start and non-Head Start children enrolled. We chose to do this eight weeks after the beginning of school, because this tended to have been the length of the summer Head Start Program. We tested non-Head Start children who, presumably, would have been Head Start eligibles. I realize these children well may not be the same kind of youngsters who had attended Head Start. Nonetheless it is fair to say that, in each instance, we found that the Head Start sample, even though tested at an earlier age, scored significantly higher than Head Start eligibles who had experienced eight weeks of first grade. This occurred on all the measures we used.

We asked the teachers to name the Head Start eligible children for our comparison sample, and tried to get a kind of subjective matching. I am not convinced it was good matching.

DR. COOKE: Did you--if I understood it, you had an increment in two weeks. Is that right?

DR. PIERCE-JONES: Yes.

DR. COOKE: Did you retest your so-called control group two weeks later?

DR. PIERCE-JONES: No, we did not.

DR. COOKE: And use them--and use that as your basis?

DR. PIERCE-JONES: No, we didn't. Perhaps we should have, however.

DR. COOKE: That's a big weakness of Eisenberg's study, too, a weakness which Bronfenbrenner sort of picked apart, for there is this change. It would seem to me that would be a must in whatever control groups are picked from now on. That we don't make the mistake of comparing the second test with the control group's first test.

DR. PIERCE-JONES: I agree wholeheartedly. On the other hand, this so-called "control group" (and I would rather call it "comparison group") were older at the time of testing. They had eight weeks of some kind of schooling in the same communities and in the same classrooms as the Head Start children. Still the Head Start children did score significantly higher at an earlier age, on the measures used.

What you are really asking is, I think, a question about the effects of practice and the effects of the initial test block.

DR. COOKE: Yes. Almost more than just practice, it's the child's approach to the test too, and it's the related motivation.

DR. PIERCE-JONES: Test wisdom.

DR. COOKE: Right.

DR. PIERCE-JONES: One more item and then I will turn this over to Dr. Bob Boger who will introduce us to our current regional evaluation program.

Four months after the entrance of all these children (Head Starters and others) into the first grade, we decided that the teachers - the first grade teachers of Head Start and non-Head Start children - should have had an opportunity to get to know the youngsters reasonably well. So we sent Mr. Don Hood around the State and got in touch with teachers and had them nominate children for certain characteristics. The most important part of this survey, I think, is that some five hundred Texas first grade teachers named Head Starters significantly more often (Chi-square) for "intellectual curiosity" and for "learning proficiency" but significantly less often (than comparable non-Head Start children) for "potential educational failure."

Now, you can say as Winston Churchill once said that "the opinion polls are not worth very much." And I would doubtless agree with you. Nonetheless, we asked the teachers to name, from their classrooms, the outstanding child exemplars of each of these characteristics. Then we went back and identified the children as to whether or not they had experienced a summer Head Start.

DR. COOKE: Did the teachers know whether or not the kids had been in Head Start?

MR. HOOD: In almost every instance that I found throughout the State, teachers had been given this information by the school officials.

DR. COOKE: Had many of the teachers taught in Head Start?

MR. HOOD: Yes, sir. We made an analysis --

DR. COOKE: They had in a sense a built-in bias.

DR. PIERCE-JONES: We made an analysis for this, too. And it comes out, regardless of whether the teacher had worked in Head Start or not, that the Head Start children are named significantly

more often for intellectual curiosity and learning proficiency.

DR. GORDON: Has anybody independently made an assessment of these kids that would support this kind of opinion poll?

DR. PIERCE-JONES: Not at this point. We hope to get to this, and we do have some follow-up work beginning.

DR. GORDON: In the Wolff study there was a similar teacher study with teachers having considerably less knowledge of prior Head Start experience and it worked out much as you say.

DR. ZIGLER: It is also significant - and I think this might be a geographical matter - that we have a lot of teachers who hate Head Start; that "halo" is in the opposite direction, so it's not a clearly contaminating feature.

DR. PIERCE-JONES: And I think we'll be on the road to getting better data fairly soon. I would point out to you that our studies involved forty communities in this State. They were not limited to Austin, or Houston, or some other urban center. We obtained the list of the Census Bureau's sample communities for the State, and then we sampled representatively from the Census Bureau's sample. In other words, we got a random sub-sample. As far as our spread around this State is concerned, we were in every part of it - out in East Texas, and out in El Paso, and everywhere in between.

DR. CALDWELL: You made one point. You said we identified these children. Did their teachers have any idea that we were seeking out these students?

DR. PIERCE-JONES: No. No. We took explicit precautions to get this information free of - what shall I say? - their knowledge that we were probing for Head Start associated effects. Overall, then, our summer '65 studies - with all their freely admitted inadequacies, which I know as well as anyone, with our logistical problems, training problems, and so on--lead me to think we have some evidence for this State, at least, that the summer Head Start Programs did make differences in youngsters in the things we were able to measure.

One could ask, however, whether or not we measured the right things. This is a problem that needs considerable discussion, somewhere, sometime soon. We have used the Stanford-Binet Scale mainly because it's available, and useable, and proficient examiners can be had. But, goodness, gracious, it may not be measuring the things we want measured with these children.

DR. OSBORN: Did you do as Wolff did? Did you measure the children again in February or March to see if they washed out with the control group?

DR. PIERCE-JONES: Did we retest them at a later time to see if the effect washed out as Wolff has claimed? We did not. What we did was to cast our money in the direction of getting Head Start eligibles, as a kind of comparison group. Frankly, I didn't anticipate the washout effect that Wolff found, but I should have expected it. It's completely naive not to think of it.

DR. OSBORN: In March before this started, I did send Mr. Sugarman a note predicting the washout effect on IQ, because Wellman had found this in the late 20's and 30's. So it's not surprising to me anymore than it's surprising that a Ph.D. will take an eight weeks intensive course in German and a year later not know a thing about it.

DR. PIERCE-JONES: It is not surprising to me either. I think what this does point out again is the need for longitudinal studies of a very careful sort. I am not convinced yet by anything that I have seen that the washout effect is a permanent one. It may be merely an artifact of some sort.

DR. GORDON: I am glad you added that because the evidence is not one sided.

DR. PIERCE-JONES: No, it isn't.

DR. GORDON: We have at least three studies that show gains.

DR. COOKE: It seems to me that it's awfully critical to get at why the differences exist between the various studies with regard to the "washout effect," because the whole basis for the so-called "follow through" effort rests upon finding out what happens to these kids who washout in one place and don't washout in another place.

DR. ZIGLER: That's a good point, Bob. And I think, you know, Urie (Bronfenbrenner) is trying to go through the studies and washout all the "washout effects." I think he is doomed to failure, and I think what we have to do is quit fighting washout effects and start studying interaction effects.

DR. PIERCE-JONES: Exactly.

DR. ZIGLER: And for us to argue that the "shot in the arm" is a wonderful thing, does an injustice to what we should be doing for kids and we've got to start looking at the interaction effects. I think your statement is directed at this very point.

DR. PIERCE-JONES: I think this is a crucial matter.

DR. ZIGLER: This is the next step.

DR. PIERCE-JONES: I have certainly not "covered the waterfront" of the studies that we have made in fifteen months but these are some of the highlights. Perhaps we ought to move now to reviewing how we have implemented the evaluation design for this year involving the year-long Head Start program. For that, I am going to turn to Dr. Boger.

THE SCOPE OF HEAD START EVALUATION

Robert P. Bager
The University of Texas

To get a feeling for our Center, it might be well for you to get an idea of our organization, or structure.

Dr. Pierce-Jones, of course, you know. There are also four other Ph.D.'s on the staff. Dr. Linn and I have been involved in both the evaluation and the research efforts. Dr. Linn will also speak in a few minutes about some of our evaluation work.

Miss Lynda Painter is really our "right arm" in the evaluation. She's our Coordinator, and she probably knows more about the various Head Start centers in depth than anyone else present. Her background is in sociology. Dr. Caldwell, who has spoken already, a little bit, is known to some of you through his authorship of the Observer Rating Form (ORF) which is now being used nationally as part of the teacher behavior assessment program. Dr. Caldwell has, of course, been involved in our Head Start research from the beginning. He was the Associate Director of our initial summer 1965 study. He continues to be a very valuable consultant concerned with most everything we are doing.

Dr. Gotts is with us this spring and is directing a study which he will speak about this afternoon. He, too, has an excellent grasp of the problems in our area. Dr. Gotts has been helping with the design of others' studies as well as conducting his own.

Mr. Swen Helge has been my "right arm" in the research we are doing in teacher attitudes. He is a Research Associate and is working on several of our projects.

Mr. Renato Espinosa is also a Research Associate with us. He has been especially valuable in developing and working with some of the Spanish language test forms. One of our significant sub-populations is, of course, the Latin American group, and we have tried to deal with the language situation in centers in which the children are not bi-lingual. We have actually used Spanish language forms of various tests. The other sub-population in the same position relative to our work is the Indian population. Here we have been unable

to make progress. Indeed, we have had to drop all Indian centers from our sample where we didn't have a bi-lingual situation. It was our decision that to bring in an interpreter to try to help in testing would vitiate the results, so our decision was not to do this.

Martha Beasley Haer is another of our Research Associates, and Mr. Clint Schuhmacher has been active with us as a computer programmer and analyst.

Dr. Grover Cunningham, is the other doctoral staff member in our Center. He is also an attorney and a certified public accountant and that is certainly more help than you can know. Dr. Cunningham is vitally involved in data analysis and the setting up designs; we depend on him in the research design and statistical areas, particularly.

DR. PIERCE-JONES: I'd like to mention Mr. Don Hood, who was here a few minutes ago but who had to leave. He was with us all of last year, and he handled all of our field activities. During last summer, ETS got mightily interested in him and offered him a salary that, on our contract, we couldn't match. Now he is Southwestern Regional Representative for ETS, but works with us also. Mr. Hood works very largely in the area of testing Spanish speaking children. He has command of the border dialects, a most helpful attribute in this part of the country.

DR. BOGER: I don't think we should omit mention of Mrs. Coody. Martha Coody is our office manager; without her we could all pass into disservice.

DR. PIERCE-JONES: Mrs. Coody helps to minimize chaos in this organization. And Mrs. Florence Bonner, who has been helping with all those name tags and so forth, is a vital member of Marty's staff.

Well, without further ado, let me get into a rather brief overview of our evaluation program. Please excuse me, if I read to you, but in order to make sure that I have covered everything, it might be better if I followed these notes very closely.

Following the revised guidelines set forth in our revised contract with IED, a limited number of Head Start Child Development Centers were selected for our sample with a view to making a relatively thorough evaluation of each such center. We selected five programs, or centers, on the basis of geographical location, the ethno-lingual sub-populations involved, and urban-rural classification. In the case of the Indian centers, where we could not test within the first four

weeks, the ability of the children to communicate in the English language was taken into account. Centers which we could not test within the first four weeks of operation were dropped from consideration as were those that did not have at least five classrooms. Five classrooms were chosen in each center stratified only on the basis of the ethnicity of teachers in order to give maximal opportunity for sampling pertinent variations within a given Head Start program. It was felt that fewer centers, with at least five classrooms, would give a better view of the centers involved. Within these five classrooms, ten children were selected for inclusion within the sample. These ten were stratified with respect to sex and age. As far as age was concerned, we simply took a median split, and randomly selected from the top and bottom of the class.

One center each was selected from the States of Arizona, New Mexico, Oklahoma, Texas and Utah. Again the idea was that we cover a very large geographical area and we wanted to try as much as possible to sample across it. Since we were only taking five centers, it worked out because it turned out that Nevada had no existing program that had been funded, at least within the requisite time span, where we could do an evaluation. It had one program in Las Vegas, but it had already begun much earlier. That left us with programs in each of the five other states, and we sampled one program from each of these states.

DR. ZIGLER: I am worried about the generalization problem. Of all of the Head Start Centers in the region, what is the distribution of number of classes within centers? I mean, there may be characteristics that might not be generalized to centers having one class or centers having twenty classes. Do you know what the distribution is?

MR. SUGARMAN: Actually, the median center size is under sixteen. This figure, you are dealing with centers that are at least seventy-five.

DR. BOGER: Right. That's true.

MR. SUGARMAN: It may be different in this region. I don't know.

DR. BOGER: Once the revised guidelines came out with the intention to sample a few centers, and a larger number of students, in order to come up with the total sample of two hundred and fifty or three hundred children (our sample size) this was the split as it came out.

There is no question about the fact that our procedure did leave out many centers that did not have more than five or more classes. This is one of the problems of this design. It probably, I think, concerns other E & R Centers as well.

Do you think that is a fair statement?

DR. GORDON: This was a fairly deliberate decision. What we did was to decide to forego generalizability for a much more intensive study, again trying to get closer to an interaction type study. Clearly the data from this year's operation will not give us the same freedom to talk about Head Start as a national program as we had hoped earlier data would. But I suspect it will give us the opportunity to make more specific statements about the relationship between what happens in the classes and what happens to youngsters.

DR. PIERCE-JONES: One other point I would like to add, here. We are so accustomed to thinking of states as units, but I am not convinced that a state is a meaningful unit for the analyses of social action programs. We decided to treat our region as a region. The variables that we thought were the important ones to look at were the sexes of the children, the ages of the children, their ethnic and language backgrounds, rather than to be overly concerned with such a question as, "Do you come from Utah or New Mexico or Arizona or Texas or Oklahoma?"

DR. BOGER: Those frequencies that you were asking about: Linda has something on that.

MISS PAINTER: When we started to see what centers would be used, we found there were twenty-five centers that had started within a reasonable time period. Out of those, only six had fewer than five classes.

MR. SUGARMAN: In this area, you mean?

MISS PAINTER: For this area.

DR. BOGER: I think, again, there would probably be a difference between the year long program and the summer program.

DR. ZIGLER: Well, for just the purpose of national planning, Dr. Gordon is right. This decision was made and I sat in on it and we had certain purposes in mind. You don't solve everything in one fell swoop. But I think we have to be sensitive to what we may be missing.

There are many variables that are of tremendous interest to Head Start. One such variable would be the number of classes. And, you know, if we are going to start doling out work and contracting and doing one thing or another, I think it would be very important to start investigating whether this is, indeed, a variable. In terms of national planning, it may be that we don't want places where they could only scrounge up twelve kids. It may turn out that these are very bad places for some reason or another. Or they may be the very best places where we may want to decentralize the centers when they get over a certain number. These are the very practical kinds of questions that we would like to see addressed in terms of policy making.

MR. SUGARMAN: One clarification on the centers. If ten children are selected from a class of fifteen that's roughly two-thirds of the class.

DR. BOGER: That's right. In other words, in the centers that we go into, we get a quite adequate sample. We really get a view of that center's children. Whatever our data gives us when we can finish analyzing it, we should be able to speak about that center without very much question.

DR. ZIGLER: This is another thing that came up in this last research meeting in New York. And I am still of the strong opinion that if we are really interested in interaction, classes, and one thing and another, we won't want to reduce that error term by running all of the kids.

DR. COOKE: It really doesn't make much difference whether you run ten out of fifteen or fifteen out of fifteen.

DR. ZIGLER: The argument was presented in the other direction and it made very little sense to either you or me, as I recollect.

DR. PIERCE-JONES: Well, I think it would be ideal if we could test all the children in the classroom.

DR. ZIGLER: Yes. Since the big work is getting there, making the arrangements and all of that, to leave five kids untested, when those five kids could make the difference between a significant finding and a non-significant finding, makes little research sense.

DR. COOKE: Do you know anything about the stability of the enrollment of the centers? Are the kids there for the whole length of the Program?

DR. BOGER: Yesterday I was on the phone with Sarah Harvey of Michigan and she asked me about the attrition of our sample. She mentioned that hers was running high or relatively high. My impression was, and here I must refer to Lou and Linda, but I don't think that ours has been very high.

MISS PAINTER: It depends on the center. In some centers we just are closer to the four week period, and, of course, we are going to have more dropouts than if you tested nearer the end of the four weeks. A lot of them came the first week or the first two weeks and then seemed to drop out.

DR. BOGER: But as of now, what would you say--again, putting you on the spot--what would you guess our attrition has been in the centers where our samples have been selected?

MISS PAINTER: I wouldn't know about that, but in Oklahoma, we had no dropouts. In New Mexico, we did have one.

DR. BOGER: Well, my feeling was, that it was under five per cent, which I thought was fairly low.

DR. COOKE: You know there might be some advantage, not to pick up your other five kids in a class, though, Ed. If you are concerned about giving tests, about what testing might do to the performance of kids taking tests. You could use the other five by picking them up later and see whether or not there is a significant difference between the kids who are getting tested the second or third time and the ones that are just jumping into it for the first time.

DR. GORDON: That would be using them in a constructive way, but, right now, they are not being used at all. I would much prefer another kind of control for this where you give over a whole class. This would present some other problems. But right now, those five are wasted either for the purposes you suggest or the general purpose of finding out whether some kind of classroom teaching within a center makes a difference. You are simply throwing away some of your sensitivity.

DR. COOKE: The great advantage of doing what I suggest, though, is that, at present, when we end up with most of the findings of a Head Start school, we attribute changes to their experience in the school, not to the test. This strategy would make it possible to get rid of that particular problem.

DR. ZIGLER: But there are plenty of studies in which you would have a good control group.

DR. COOKE: There are good studies, but I am not sure that there are good studies with this population. You see, in taking a bunch of middle class kids and giving them a test and then testing them again you don't see very much change in performance. But suppose you take a bunch of kids who are not middle class and who have very different ways, maybe, of looking at the test, maybe they worry about it, you know--you can make up a lot of reasons--and then they take it (the test) again. Perhaps the second time around they have a lot more confidence, et cetera, et cetera. The results could be quite different from the so-called well known studies.

DR. ZIGLER: Yes. I have done the most complete study of this sort, myself. I test the children in many different ways, and retest them, and have all of these kinds of controls. Not only do I retest them--these Head Start children--but I do it with a different kind of test, one which pulls out what everybody is calling the practice effect, test taking attitude, or one thing or another. I personally know that, even if you control for these factors, you still get increases. I also know, at least on the basis of our findings, that the effect that you get has nothing to do with intellectual improvement. The changes have to do with the kinds of things that John was talking about earlier - attitudes, motivation, a variety of things which seem to last and stay in these kids pretty long. I think that is what John was referring to. We may be looking at the wrong thing.

DR. PIERCE-JONES: That was exactly the point I was trying to get at.

DR. ZIGLER: So there is this kind of work. That doesn't mean there couldn't be more of it and that those five kids couldn't be used in exactly the way you are suggesting. The control problem has been a serious one. The first summer, we did not even have a control group. But things are shaking down, and people are going to start doing that kind of thing. The very best people have always done it. There is nothing terribly new in having a control group that has both sets of tests that your experimental group does. That's just a simple thing. Unfortunately, some very simple things have not been done.

DR. BOGER: One other thing concerning the kinds of factors that were uncontrollable but which did structure our sample: we were going to try and do our pre-testing within the first four weeks.

That meant that those centers that had begun before we were funded and operational, (prior to three or four weeks before we got underway) were out because we couldn't get to them in time. Those that were funded later, we could use if, in fact, they were going to get started and if it looked like their funding was going to go through.

Now, this has been one of our problems even here in Austin--this is one of our sample centers--because we can't tell whether or not they are going to run past the end of April, which would be only, approximately, three months. Austin is questionable at this stage. At least, it hasn't been firmed up. These kinds of things, of course, are always hard to predict and to deal with.

MR. SUGARMAN: I don't understand why they wouldn't run past April.

DR. BOGER: Well, their funding only took them through April and the last we heard, it had not been extended and there was a question--

MR. SUGARMAN: It wasn't funded?

DR. BOGER: I don't believe so.

MR. SUGARMAN: Let me ask prospectively. Some of the programs, and I presume you in this region as well, are funded twelve months a year. Now, looking in the future, at what point would you expect to test in those programs?

DR. BOGER: Well, my reaction here would be as soon as possible. We should do our pre-testing in the first week, not the fourth week. We should get to the children immediately. This year we couldn't do it. By the time we were operational, and things were settled down enough to know what we were going to do in terms of sampling and so forth, we couldn't do it.

MR. SUGARMAN: Would you say the first week as against pre-entry into the program?

DR. ZIGLER: Yes, that's what I was wondering. The optimum is before they ever show up. The findings are indicating a tremendously big "first week effect." You are underestimating.

MR. CARLTON: Well, if your biggest attrition is during your first week or your first two weeks, isn't there a built-in factor that will slide you down hill?

DR. OSBORN: Yes. But if you have this thing appearing that Dr. Pierce-Jones mentioned earlier, of this thing appearing in the first two weeks, if you are in the third week, you are in trouble already.

DR. ZIGLER: The optimum design requires that you get yourself a trailer, or a storefront, and test the kits before they ever show up at the Head Start class.

By the way, this other point always seems to come up inadvertently, that the operational arm of Head Start and the research arm haven't worked closely enough together. Now, you know, much waste can be involved in an operation like the Pierce-Jones operation, if a Head Start Center like Austin, which is one of his sample centers, is not funded. This is totally ridiculous. I think it certainly would take a very small policy decision on your part to have the various E and R Centers let the operational arm know which Head Start centers are being used in a research enterprise. Those centers could be "red-flagged" so that they would get special consideration. We've already got such a big investment in them that for this thing to break down, because they are not being refunded or something like that, is not wise.

MR. SUGARMAN: Franc, do you want to cover that?

MRS. BALZER: Research, they are coming down and telling us, should we give priority to this. And in a number of instances we have.

DR. ZIGLER: I would suggest some formal mechanism.

MR. SUGARMAN: It is now your responsibility.

DR. GORDON: Jule, we might take that even a step further. The note I made here was can we tie certain management decisions of Head Start centers where E and R Centers are being operated a bit closer to them? It seems to me that if Dr. Pierce-Jones had some voice in the beginning date of the centers, so that if it were necessary in order for them to make good research to postpone that date, we can. We ought to have some way of establishing this.

MR. SUGARMAN: I have a reluctance on that one.

DR. GORDON: If we are talking about getting kids before they actually get into the program, this becomes essential. Just getting our research centers in contact with them during the first two or three weeks is a problem at this time.

MR. SUGARMAN: Well, assume that your sample remains fairly stable in terms of which centers you are working with, I think that you will have established an ongoing, working relationship with those centers so that they can tell you pretty well what their plans are. And then there needs to be some adjustment, I think, on the part of the research staff.

DR. BOGER: Again, though, because of the way in which we had to do things this year, I am not so sure that perhaps the sample centers we are working with are necessarily the best sample centers to represent our region. When I said the first week, I meant that since we have had trouble arranging to test in the third week, the first week seemed to be optimum. But it, of course, wouldn't be. If, somehow, the sample could be drawn by the middle of the summer, let's say, and if we knew at that time that the chosen programs were going to be funded, we could begin working with these people.

MR. SUGARMAN: Well, there is a general policy rule operating that says that our objective is continuity of program. And therefore, first priority of funding goes to programs which existed before, if they meet two conditions. One is, that they are serving the right kids, not serving nondisadvantaged kids. The other is that they are operating a satisfactory program. So you can almost assume that each center you now have, if it is operating well, is going to be refunded. You can lay your plans on that basis, I think. Now, you may get foul-ups in the paperwork refunding and that could affect the starting dates, of course. But if you stay in close touch with the centers, I think you will, by and large, solve that problem.

DR. BOGER: Knowing actual starting dates is, of course, critical.

MISS PAINTER: That's been a problem, because a lot of the centers we thought were going to start in November, didn't.

MR. SUGARMAN: Yes.

MISS PAINTER: Like Austin. It was supposed to start in November. You know, they said they were going to start. And they actually started on January 25th.

MR. SUGARMAN: Yes. But for this year, for most of the-- many of the---centers, it was the first date of starting. Now they are beginning to become an ongoing institution and you should have less problem. Although, albeit, we still have problems. But if

John will tell our field liaison people which centers are involved, then we can pinpoint those.

DR. BOGER: I was standing here thinking, I wish Shirley Powell could be here. She's got her tremendous headaches in this, too, in trying to get this done. It's not a simple procedure.

DR. OSBORN: Who is Shirley Powell?

DR. BOGER: Shirley Powell is the regional coordinator for Head Start in the Southwestern Region Office here in Austin.

DR. GORDON: Before we go on from this point, there is another kind of administrative relationship and device that might improve this thing. When we talk about really serious things such as control groups, it would be ideal if a certain center had the privilege of assigning this group from a pool of youngsters and assigning other youngsters from a control group. Now, I know that this is completely impossible, at this time. Is there any possibility in a spot like Austin now, that we could generate a pool of youngsters, all eligible for Head Start. From this pool, some youngsters would be assigned to Head Start and others not?

MR. SUGARMAN: If you can get a--

DR. GORDON: It would involve a very active type of center. There is no question.

MR. SUGARMAN: Are you saying, Ed, that you might recruit say a hundred youngsters from which fifty are selected to go into Head Start programs and fifty are selected to not go into Head Start Programs? That, I think, presents certain ethical problems. If you have a hundred kids, and you have to make a selection of fifty because that's all the slots you've got, we really would like you to see the fifty kids who need the program most would be selected. That immediately throws off any comparisons that you might make between the selected and non-selected children. I'd really like to have the judgment of the Planning Committee on that kind of approach.

MR. CARLTON: If you are talking in terms of a hundred kids from Austin, I don't think you would be able to distinguish which kids would need the program the most.

DR. COOKE: Yes. You know, I think you could just randomize a hundred if you say that you don't have enough. In East Baltimore, we figure out we have got three thousand kids that ought to be in Head Start and we've got enough money for five hundred. So there

are a lot of controls lying around. Now, if you could get these, and simply randomize the group, I would think it would be most helpful. But then you'd need to make measurements on your controls at the same time.

MR. SUGARMAN: I think the problem is that we really push the idea of communities picking their hundred slots or five hundred slots from the three thousand on the basis of need for the kids.

DR. COOKE: Now, but I think the statement made is that we looked at three thousand kids.

MR. SUGARMAN: Yes.

DR. COOKE: You would really have to work like the devil to be able to say which of the three thousand ought to get it the most. I think you've got a lot of kids that need it far in excess of the numbers that can go, so I don't think you are really going to get much by any very intensive selection process. My own guess would be that it wouldn't pay even to try to do this kind of intensive selection, because you would end up with so many in the group that ought to go. However, if you get to the point where two-thirds of the kids are covered by Head Start, then there is no problem. I mean, there you would have to adopt a selection mechanism.

MR. CARLTON: Well, if you let natural events take their course in that group of three thousand, you will find, I suspect, that the kids whose parents are aggressive about it would be the first ones to go.

MR. SUGARMAN: Well, let's take this simply. You've got five hundred slots in (East) Baltimore? And you've got three thousand potentially eligible kids. In the natural course of events, people will wander in off the streets and sign up for maybe a thousand. Now, you could just stop right there--

DR. COOKE: I would have an active recruitment program to get my three thousand--

MR. SUGARMAN: Now, after you get the three thousand--

DR. COOKE: Then you would randomly take five hundred out of three thousand. You see--I don't mean you would just take the first five hundred that walked in. That's what has been done in the past.

DR. ZIGLER: That would be a good design. The two designs that would be bad designs would be if you obviously, put the kids that needed it the most into Head Start and left the kids that needed it the least as control groups. They would no longer be control groups. The one in the opposite direction would be the one that you are worried about, Jule. It would involve taking in the kids who came to sign up and then saying, "okay, our control group is going to be kids who don't come." Well, you have a bad control in this case because you've got a selective factor, and, as you point out, parents that don't want their kids to go. And the kids are different. So that's a bad control group. There are two possible control groups and, I do think, it does demand a policy decision.

The first control group is one that Ed Gordon is speaking to, where you have a hundred parents show up that say, "I want my kids in this thing." And you say, okay, we are going to put fifty in and fifty out either random or else a selection procedure in which you have some variable controls. Now, that's where the ethical problem comes in because both kids need it equally badly. The question is: do you want to follow this kind of procedure?

DR. NIEMEYER: Do you have room for more than fifty?

DR. COOKE: No.

DR. NIEMEYER: So in case you have room for fifty, Jack --

DR. ZIGLER: Well, let's say you have room for a hundred.

DR. NIEMEYER: And you have room for a hundred?

DR. ZIGLER: And you have room for a hundred.

DR. NIEMEYER: That's a very different ethical problem.

DR. ZIGLER: Of course, it is. But let's say you have room for a hundred--

DR. NIEMEYER: And a hundred people show up?

DR. ZIGLER: And a hundred people show up and you want fifty in your control group that don't get it. Well, that is obviously important, and so--

DR. OSBORN: I'd like to make a policy decision here. If we have space for a hundred and a hundred show up, I'd say the research group goes and finds another community.

DR. ZIGLER: Well, let me tell you why that is a better control group.

DR. OSBORN: Well, that's--

DR. ZIGLER: Well, let me finish my spiel. It turns out that what always appeared to us to be a perfect control group is not a good control group because of "ripple effect." It has now been demonstrated by Susan Gray that the fifty kids in your experimental group interact with the fifty kids that are not in your experimental group. And what you get is an underassessment of the magnitude of the differences between your two groups. The optimal control group is one in which you go to another community where there is no contact. This gets you away from the ethical problem altogether. Of course, it's difficult to do this. It costs more money and you have to make sure that the other community is like your first community.

DR. SOGER: And to match populations is particularly hard.

DR. COOKE: I would think that this ripple effect is going to be almost infinitesimal if you pick a hundred kids out of three thousand, that is, where you have thirty times as many kids in need as can be served.

DR. ZIGLER: Then you would have no problem.

DR. COOKE: There would be very little.

DR. ZIGLER: This does have some implications as far as support of the regional research center.

DR. COOKE: It sounds to me as though they need some kind of recruitment money. You have got to have some staff that can help out. I mean recruitment for patients or subjects. (Laughter)

MR. SUGARMAN: Are you talking about the research center or the operating center?

DR. COOKE: I am talking about the Research Center. It has got to be able to help the local group recruit many more children than they can serve. You have to do that if you are going to have this appropriate design.

MR. CARLTON: It just has tremendous political impact when you go out and recruit three thousand kids when you know you can only admit five hundred. That, generally speaking is not good business.

DR. COOKE: I am not sure it isn't the best possible business in the world. It really shows the need and in the long haul that's what you are going to have to demonstrate.

DR. CALDWELL: Then what is your basis for selection?

MR. CARLTON: Well, theoretically, the basis is that you get a list of all those that come in on their own, plus the ones that you have gone out and knocked on doors to find using whatever community aids or social workers and welfare staffs and so forth that you have. And then somebody, "somebody" being the staff of the Head Start Center has to make a judgment as to the relative needs of the kids involved. I don't really think it's operating that way at this point. But it's probably what we are trying to push in the direction of.

DR. COOKE: You are doing the same thing then. You are turning some people away.

MR. CARLTON: Yeah, but we don't go universally and we sort of control it.

DR. COOKE: Well, let me say that I think this would be entirely all right except (or only?) where you've got a Research Center. As a national policy, that is the worst thing you could do. But what is the coverage of kids who are being supervised by Research Centers in comparison with the total? It's a very small part of the total. But you are not really, politically, stirring up hornets nests all over the nation by having many more kids in line than you can service. If the setup is designed to have some real meaning, I think you've got to have control customers who can be studied and the experimental groups representative of the whole bunch. And the only way to do that, really, is to over-recruit and then randomize.

DR. GORDON: There is another approach, and that is to get out and recruit youngsters for whom we have no facilities. If you would agree that we no longer have to debate the question of whether or not it pays to have a Head Start program, but have moved to the level of what kinds of programs are most appropriate, then we recruit pools of youngsters and assign them all to specific treatments and make our comparisons between treatments and non-treatments and no-treatment. And this is the present focus of your program. I really don't think we need to go back and argue the question of whether or not the youngsters that don't get anything do just as well as youngsters that do get a good program.

DR. ZIGLER: Well, I have heard this before, but I don't agree with it, for the simple reason that we don't know that much. And you might get no differences between different cognate treatments. But everyone of those treatments may be better than no treatment.

DR. COOK: One of the things you learn here is the kind of child that might not need this. We are going to get to that. We are going to have to get to that one of these days. So what you may see in your control groups that haven't had anything is that the children seem to come from the same group as those that need and get it, although his performance is entirely different.

DR. ZIGLER: I would say one other thing to the ethical problem. I do think that we need the control group. The kind that Bob is talking about is absolutely the best kind of providing. Again I am concerned about "ripple effect" and making sure the children are in different parts of the city and not interacting.

The other possibility, though, and we've done this, not in Head Start but with older kids where we've set up control groups exactly this way. In the Grant program, for example, older kids tutor or don't tutor, then, once the experiment is over--it's usually about a year's duration - you move in and do something with the control group. In other words, they may go to the first grade, but you provide something for them too and for their parents for having been in your group. Learning isn't over by kindergarten or the first grade, I guarantee you that. So what you tell them is, in effect, "We are putting off your 'goodies' until next year if you help us with this."

This doesn't take away all of the problems, but if you are terribly concerned about ethical considerations, this is one way of at least watering it down a little.

MR. SUGARMAN: It does contaminate your longitudinal measures, though.

DR. ZIGLER: Yes, it does if you are--if it's a one year program you can do that. If this is your group that you want to watch for ten years, obviously, you cannot do that.

DR. BOGER: This, I think, is the critical factor in my mind, at least. As I hear you talking about this, once we reach a position where we can get this kind of sample and this kind of design, we would have the base on which to build the kind of longitudinal study which I think is going to yield eventual answers to questions as to whether or not these kinds of programs really make any difference. If we were

in the fifth year of a study such as Dean Holtzman was talking about this morning, and if we had a good foundation in that first year and a good design, then we would know a great deal. Until then, really, we won't.

MR. SUGARMAN: And how much of an attrition loss can you take with a group of two hundred and fifty in order to maintain statistical meaning?

MR. SCHUHMACHER: Of paramount importance, is knowledge of how strong is the effect that you are trying to measure? You are going to have an awful lot of trouble if you end up with any less than a hundred people when you get through. If you have any less than that, you probably--

DR. COOKE: If you have that much of a dropout, you are going to have so many other effects that are going to overshadow the thing, for example, what kinds of individuals drop out and so forth.

MR. SUGARMAN: I was thinking of a more long range basis-if you are watching for five years.

DR. ZIGLER: You've asked an impossible question. If you are looking for interaction, you have to start talking about your individual cell meaning and the contrary. I mean, if your design is a simple two-group design, that two hundred and fifty people could drop down to sixty and I don't think it's going to be totally harmful.

DR. PIERCE-JONES: You're still in pretty good shape, except that the attrition is selective rather than random.

DR. COOKE: That's what shifts things so.

DR. ZIGLER: That's another thing, I think, that is going to have to be built into the research program. Not enough is being done about attrition. The studies that are now being supported in the bigger cities show that exactly half of this population moves during the course of a year. That's a constant figure that has been showing up.

DR. COOKE: But they don't move far.

DR. ZIGLER: They don't move far. And the thing about too many researchers, I have discovered, is that once they move out of the school, even if they move six blocks away, they are lost. Well, they don't have to be lost to a good research group, that has the resources to follow them around. It could add to the problem where you have got shifting treatment effects.

DR. COOKE: When we followed up (we've followed several thousand children, now, for a period of from four to seven years) we've retained ninety-three per cent of the children.

DR. ZIGLER: These are poor kids?

DR. COOKE: Oh, yes, These are kids in the East Baltimore area. They were enrolled in the Etpalmor Project. But it's an awful job. I can tell you that. One of the most expensive types of study is to maintain this kind of follow-up. The places that haven't done it, have lost as great as thirty or forty per cent. But you can keep in touch with them.

DR. OSBORN: But the point Ed makes is true. With the poor, it is seldom that they are moving two thousand miles away. This could happen more often to the middle class. They are very often moving down the block.

MR. SUGARMAN: To a different landlord.

DR. COOKE: Yes. And they do that several times a year, but once they enter the school system, there is absolutely no difficulty in tracking them.

DR. BOGER: Once you get your system set up. And, of course, some people in Iowa, and we have gotten data from them, in their initial year of Head Start research, did some work on setting up a system for this. I agree this can be done, and it is being done. Virginia Shipman in Chicago said, I believe, that they have maintained ninety-six per cent or ninety-seven per cent of their sample over a two or three year period. In the Southwest, it might be even easier to do this because there is going to be less possibility of loss within the smaller communities. There is a larger rural population within our total population and even our urban centers are not as large as elsewhere.

Another thing I am impressed with (and I think that this is important in light of this conversation) is the real interest and cooperation we have had from the Head Start centers. Even though they may not entirely understand the kind of design and sampling problems that we face when we bring them to them and explain them they are at least "in on" our experience.

I think you would get this same cooperation if, in fact, the centers were pre-selected and if we said, to them something like, "Look, you are going to be part of a longitudinal, long-range study."

I really think that, right down the line, the only problem we might have would lie in trying to explain why someone fifty miles away weren't in it.

DR. ZIGLER: You know, I think that again, we kind of left the policy decisions up in the air. But the point that was just made came through to me loud and clear in Urie Bronfenbrenner's letter, too. And this is that, during the very short time that we have been in business, there has been a change at the community level about the attitude towards the research in Head Start. They couldn't have cared less whether I was a professor or not.

At any rate, I was very concerned early in the game, about the apathy, the reluctance, the defensiveness. But now what you get is, well, I don't know if you know or not, but scores of letters have poured across my desk asking for the use of this inventory, which I sent along. Urie has picked up the same sort of attitude changes in his travels, and I have also picked them up, and we are talking to them here. I think, therefore, that in making a decision like this control decision, it has to be a policy decision. I think we should take the community acceptance factor into consideration. People now seem to be ready for this thing. They want it evaluated. They want to see how well they are doing with these things. They are much more receptive to it now.

MR. CARLTON: They were actually receptive to it in the first summer, except, just by the very nature of the way things developed, some rejection developed. As you remember, you met like on Friday, when schools were starting on Monday, and they hadn't heard about the evaluation program in orientation. I think the teachers would have been more receptive had they known more about what was going on. But, you know, they were asked to give the tests themselves, and they didn't understand it. The tests appeared late. You know, there were so many demands.

DR. NIEMEYER: Bob, I am aware of the fact that we are--Bob Boger is trying to make a report to it. And I don't think this is a proper moment for me to bring up another question. But I hope on the agenda today, can be an organizational question which is the relationship of the centers to the regional OEO and Head Start people. I think it would be appropriate for us to have some discussions.

DR. BOGER: We will speak to that in a moment.

MR. SUGARMAN: Well, let me make an observation that in general these dealings have been direct from Washington.....

DR. NIEMEYER: Many of the questions that have been brought up here suggest a need for some kind of liaison or joint policy or at least, joint discussion advisory committee including not just Head Start and OEO but community and so on.

DR. BOGER: I see no reason why we don't jump in to it right now.

In terms of our liaison with these centers, basically, they have been very fine as we had expected they would be since we are both doing the same basic job. What we did was to contact each pertinent regional office directly. Our Center, because of its large geographical scope, has had actually to relate to three regional OEO offices. Dr. Luevana of the Western Regional Office in San Francisco, Mr. Rashid of the North Central Regional Office in Kansas City, and Miss Shirley Powell, Head Start Coordinator of the Regional Office in Austin. They were all most cooperative. In fact, we would ask for things that I knew took them some time and effort, and we would get them the next day. They clearly went out of their way to meet our needs. The only reason that more communication hasn't taken place with them is that we have been extremely busy getting the job done, as have they. Now that we have a years operational experience behind us, and because of the various design problems we have been talking about here, perhaps we should get together with operational people to share their concerns and those which the research arm has. In other words, I am sure that both sides have information that would be helpful to the other.

Unlike some centers (for example Sarah Hervey's at Michigan State, whose regional office is in Chicago) we are very fortunate in having our regional office right down the block, as it were. For that reason, Shirley Powell has been able to do things for us that other centers, I am sure, haven't been able to get done for them.

DR. NIEMEYER: I am thinking that the committee might turn its attention to the question as a national issue because you've adjusted a number of the complications that are in the picture and that the committee is trying to work out.

MR. SUGARMAN: I will try to do that sometime.

DR. COOKE: We can do that tonight.

DR. BOGER: Let me move on through now and give you a brief overview of what our sample is like.

The rural sample comes from the Salt River Pima-Maricopa Indian Reservation near Scottsdale, Arizona. These children are all from the Pima-Maricopa Indian tribe and are bilingual. Factors that I mentioned before influenced the selection of this particular reservation; it was one of the few bilingual ones.

The Head Start program in Anadarko, Oklahoma qualified as a small community sample, having a population there of about sixty-three hundred people. The program was of special interest to us because of the involvement of the Negro, Indian, and Anglo children. Because of some research that we have done in the past, we wanted to get certain centers where we could get more information concerning the idiosyncrasies of some of these sub-populations and the kinds of needs that they have.

The third center selected was Carlsbad, New Mexico. It has a population of twenty-two thousand, which was a small city sample. The children involved in the program are predominately Latin-American. And a high number here required testing in Spanish. When necessary, we tested in Spanish. More will be said a little later concerning the Spanish dialect forms of the Stanford-Binet and Pre-School. Mr. Hood has been vitally involved in this, as have Mr. Espinoza, Mrs. Haar, and others. Suffice it to say here that Spanish forms of the Pre-School Inventory and the Stanford-Binet have been developed by our center; they are Texas-Spanish dialectical forms. We are attempting to try and develop test versions that will deal adequately with the dialectical variations in our region, although we haven't begun to deal with such linguistic matters in the Indian population. This is something else that we must attempt to do.

The remaining two programs selected were from a large and a small metropolitan area: Salt Lake City, Utah. This is the one in Utah, which is also interesting and important to us because it is a predominantly Anglo population. Austin, Texas, on the other hand, was chosen because in addition to its size, the majority of the children in the sub-population being served are Negro.

Each Head Start center selected for our part of the national evaluation was contacted by telephone and by letter in order to elicit cooperation. These contacts were followed up by staff visits during which the evaluation was explained to each center in detail. We dealt through the Community Action Program directors in each case.

MR. CARLTON: Did that pose any problems?

DR. BOGER: No, it did not. I had the feeling that there was

some concern about this at the national level, but, as far as we are concerned, it posed no problem at all. Again, I can't reiterate too strongly that when we went into a center to set things up, we were greeted with open arms. It wasn't a matter of convincing anybody.

The only problem we found and this is what I wanted to mention earlier, is the strong concern felt at all of these Head Start centers about data feedback. Particularly in the more nearly rural areas, we are gathering data on the children which is much finer than the schools have ever had before. In some of these places, for instance, to get a Binet test given is a major effort. We have all this data, but for pretty good reasons, of course, the national guidelines have forced us to have to qualify the kind of feedback that we have been able to provide.

It is very important that we follow up and establish reasonable and serviceable guidelines here. We believe that we should provide reasonable feedback to the people. Indeed, this is one thing that, over a period of time, will solidify our good relationships with them. At present, we are thinking of attempting to ascertain the "level of the feedback" that is satisfactory and ethical. We don't want to "label" individual children, but we would like to try to give information back in some useful way.

DR. PIERCE-JONES: May I add a word to this, Bob?

DR. BOGER: Please.

DR. PIERCE-JONES: After our data collection in the summer of '65, we put Mr. Hood "on the road" to go back to all of these communities with a feedback, permanent feedback. Is that right, Don?

MR. HOOD: That's right.

DR. PIERCE-JONES: How were you greeted and what is your impression of this?

MR. HOOD: People I met in each place were very surprised that anybody would bother with a feedback to them. They said, "This is the very first time that anybody has ever come to us and gotten information from us and then come back and interpreted it, to us. If you want to come back in our schools and do more work with us, we will welcome you with open arms." They were overwhelmingly in favor of our work and in continuing to work with us in our efforts to help them in their own communities. This feedback kept the door open indefinitely for us to come back.

This matter is of vital importance as we see things. We also obtained, through this personal feedback, samples of community opinions of the effectiveness of the Head Start program. How it could be changed, or altered to be more effective in the community. And then, in our efforts to work with them, we learned something about how we might exchange information better. The locally responsible people were more than happy to receive us.

DR. ZIGLER: I am a little confused on this point of guidelines and one thing and another. John, you remember the meeting in Boston?

DR. PIERCE-JONES: Yes.

DR. ZIGLER: Ed, (Dr. Gordon) I don't know if you were there. You had to go back to Harvard that day as I remember. But, I spoke to this issue and John was one of the few people who received it kindly. Yet anybody who has ever worked with kids and teachers and schools, knows the importance of feedback. There is nothing in the world wrong with this. We have a lot of Head Start money tied up in this. Is there any reason that this couldn't be partially a service enterprise for the people who are participating in it? No reason at all. It doesn't depreciate your research. And I am hearing murmurs here, I mean, because I wanted to set a guideline at that time, that this be built into the program. You remember, John?

DR. PIERCE-JONES: Yes.

DR. ZIGLER: And some of the people didn't take too kindly to that. You did.

DR. PIERCE-JONES: Well, out of our experience--

DR. BOGER: Well, I just want to say--maybe I shouldn't have used the term "guideline". Because I think really it's guidelines that partially come out of ourselves. In other words, there is a real ethical dilemma here, and you have to deal with it from a professional standpoint. In other words, these people want the scores. But, do we set up a general rule that our information is to be fed back? And how do we feed it back? Do we feed it back in the same way to all centers whether or not there are qualified personnel to interpret it or not? Do we give IQ scores out for five year olds, let's say, to file away in a permanent folder (which they will be, I think--).

DR. GORDON: There exists a set of guidelines on this.

There are two kinds of feedbacks. The one that has just been described where a center will send someone back and I must say, John, you are an exception in this case. I don't know anybody else that is doing it.

DR. PIERCE-JONES: Well, we feel it is absolutely necessary to get this done and done well.

DR. GORDON: This is the interpretation of data from a particular group of youngsters?

DR. PIERCE-JONES: Right.

DR. GORDON: The second level has to do with the release to the schools of the raw data--the actual scores. Our present policy with respect to this, where there exists, in the school, a person who is competent to use the data, the E & R Centers are free to do so. Where a request comes for an individual youngster's score, then this is with the parent's permission. Where a general request is made and there appears to be nobody in the Head Start Center who knows what to do with the data, we do not send them.

MR. HOOD: This is one reason we felt that it was important to have a personal feedback. We got from our computer, a printout of each child's scores. And in going back to the center, I sat down with the person in charge and interpreted these scores to them, because of the varying degrees of test sophistication in the various centers. I sat down with each person and interpreted the scores.

DR. COOKE: On each individual child?

MR. HOOD: We showed them how their children compared with other groups.

DR. BOGER: This, of course, is what we would like to follow up on. But, again, it's a policy matter that we have to establish, because very complex factors come in here.

DR. COOKE: Did you get parental permission? I take it you didn't.

MR. HOOD: Parent's permission for what?

DR. COOKE: For the feeding of this information to the school system? The results of the investigation? Wasn't it to the school system or not?

MR. HOOD: This was to the Head Start center. Most of the time it was provided to the Director of the Center for the children who participated in that particular Head Start center.

DR. COOKE: Now, what did they do with that? You said that the school system--I think somebody used the term, "the school system was very appreciative of this feedback." And I wondered what did go back to the school system.

MR. HOOD: The child's name, the raw scores, how long with--

DR. COOKE: That went through the school system?

MR. HOOD: Right.

DR. COOKE: Yes. So there was a feedback.

DR. NIEMEYER: I am confused. I thought you went back to the Director of the Center in which the children were enrolled.

DR. PIERCE-JONES: Oftentimes, the Head Start center is associated with the school system.

DR. COOKE: Is the same.

DR. NIEMEYER: Now, is it that Head Start center that was a part of that school system?

MR. HOOD: Many of the children were brought in from outlying districts. Then when they entered the first grade in the fall, they could be scattered back out in different places again. In Houston, for example, it would have been impossible for us to know in which school each child enrolled in for the first grade.

DR. NIEMEYER: So it wasn't a clear feeding back?

MR. HOOD: We had to take it to the administrative office and then let them disseminate the information however they wished.

DR. COOKE: Well, then the information, I am not critical of this, but it is perfectly possible that if this information went to people who might not be able to use it properly, would--

MR. HOOD: This interpretation was made to begin with and then let them pass it on--

DR. COOKE: Yes.

MR. HOOD: And then let them reinterpret it the way it was given to them. But we have no way, of course, in any kind of standardized testing of assuring that any classroom teacher is going to use information in the most effective way.

DR. COOKE: Has anybody looked into how it is being used?

DR. PIERCE-JONES: No, we haven't.

DR. OSBORN: You know, we have dealt with this for years with our children where we have an absolute rule of never giving an actual score because by the time it gets to the classroom teacher, the actual score is meaningless. We also work with clinics with teachers that got a score in the first grade of a child that had done poorly. And this score is passed on year by year and they immediately think this is a nincompoop, and treat him accordingly and before long he is. At what point--

DR. COOKE: Ed, at what--

DR. OSBORN: Wait, I just wanted to finish this one point. What we do interpret to the teacher though is more generalized data on expectations she might have and where the child succeeded, generally, in terms of verbalizing things. We have found that a conference does this very well and gets us out of the teacher saying, "Well, I have Johnny Jones. What did he do or what is his score?" Because you can't tell the difference between 108 and 112--

DR. COOKE: The facts are you can't. You can't conference with all the teachers that are involved. That's the obvious thing about this.

DR. PIERCE-JONES: We tried the strategy of conferring with some key personnel.

DR. COOKE: Yes. But the chances are that person could very well--

MR. HOOD: Also, we gave them a written report. It was written in less than lay terms so that anybody could come in and pick up this document and read it from cover to cover and understand every instrument reasonably well - what it was designed to do, how it was used, and the kinds of raw scores we received from it. And then over here with a printout of the child's name and how he performed as compared with others of his age group.

DR. PIERCE-JONES: You included normative tables?

MR. HOOD: Right. We included norm tables so that anybody with a college degree who might come into a classroom situation, should have been able to make sensible interpretations. This is why we went to such great lengths in writing the document. We wanted it to be easily understood.

DR. BOGER: One other thing that we have done in cases where we have had a question of feeding back actual scores (like IQ's) to some centers, particularly the smaller centers where people to utilize them adequately were lacking, has been to indicate that we have the information in our records. And that is another good function, I think, for the E and R Centers to exercise. In other words, we say, "We are here to serve you. We will continue to be here. These scores will be available. If you have need to know any specific information, please communicate with us, and we will provide you with a careful report based on whatever data we have."

MR. HOOD: May I add one other thing? In our personal visits, we noted considerable hostility from the school people who were expecting some sort of feedback of information on the entire Caldwell Pre-School Inventory the year before but who had not received even a word about whether it had been scored or not.

DR. OSBORN: Well, I think this is the key point on feedback. Very often, you will get the cooperation for the thing. But if you don't feed back, the next group that comes in is dead.

MR. HOOD: This is why it was imperative that we feed back.

DR. COOKE: I would like to point out, again without taking a position on it, that you can gain the psychological goals you suggested, and it is very important to do so without reporting specific scores.

The second reason for feedback is that the testing gives leads to educators about something they can do. One of the great problems with the Stanford-Binet score is that neither teachers nor psychologists, under the guise of counselors, have known what they could do about it. And consequently, I think that if we are going to get involved in the question of what goes to schools out of the Head Start program, it is the question of what can be done to give some, at least directional, guideposts to the educational establishment for something that can be done about something.

These scores often become immutable labels on children in the system. And as Keith says, they follow the disadvantaged populations so that, by and large, the teachers of the country believe that these kids are not very educable. If, then, we simply give additional support to the established emotional set in the teaching profession, we are going to do harm.

DR. PIERCE-JONES: That's right.

DR. COOKE: What do they say when you ask them why is it helpful to get this feedback?

MR. HOOD: Most of them wanted to see how their community-- well, let us see how we measured up.

DR. COOKE: So group things would be perfectly useful, then, for that?

MR. HOOD: They were wanting to know how they fared with other like groups. I happened to have been part of a groups. I happened' to have been part of a group developing a program for migrant children in South Texas a few years ago. Our Brownsville people, for example, know that they are not going to compare favorably with the Dallas group. They are different kinds of people. But according to the people around us, they ask, say up here at Harlingen, just up the road a little bit, how do our people compare with those? If our children are greatly different, where were their weak points? They almost want an item analysis as far as "developmental tasks" are concerned, so that, in turn, they might emphasize these kinds of things in their programs.

DR. ZIGLER: I would like to make a suggestion so we can move on, Bob.

DR. PIERCE-JONES: I think we should move along.

DR. ZIGLER: One thing is clear: the dangers that Jack presents are real ones and we are all aware of them. The other side of the coin, as you know, diagnosis and treatment of kids and groups and having programs, that is the good side of the thing. What worries me is that there hasn't been enough thought given to it. The Texas group looks like they have given some thought to it.

I think we ought to have a national policy here so one regional center is not going to do one thing while the other ten are not doing it. One of the orders of business for the research group should be--

to really set out guidelines that everybody can live with and that can take into consideration Jack's concerns while guaranteeing feedback, making sure that we are abiding by the healthiest set of ethics that we can come up with, protecting children from the harmfulness of the indiscriminate use of numbers, and so forth. I would think that this is something that should have a high priority of business for the Center Directors at one of their joint meetings, that is, to come to grips with this problem. They should work out a mechanism and develop it themselves. There is plenty of ethical know-how and skills in that group. I think that we should have a unified policy that includes this kind of service that assures feedback to the communities involved in our research activities.

DR. PIERCE-JONES: It pays you back a thousandfold--

DR. ZIGLER: I am sure that it does.

DR. PIERCE-JONES: --in terms of good will.

DR. BOGER: If we can move along because we are going to break for lunch in approximately a half-hour and we have got a couple of other things we would like to get at.

DR. GORDON: At the risk of slowing you up, I must make a thirty second comment.

DR. BOGER: All right.

DR. GORDON: There are a lot of very interesting and useful and, I think, appropriate ideas coming through. I hope you will remember that, at some point, it's got to be reflected in the way the centers are funded. Feedback doesn't just happen automatically. (Laughter)

DR. NIEMEYER: The planning committee is always very willing to vote money. (Laughter)

MR. CARLTON: It's interesting to see how much Texas has been able to do within the amounts available to them. (Laughter)

DR. ZIGLER: Ed's point is well taken. I think this is going to have to be one of the things that is looked at. Perhaps this feedback costs a little bit more money, but I don't think it costs a fortune. It's been pointed out, the Texas people have been able to do it. However, if it does cost some money, it's money well spent. And it has a service component that is essential. So, I think your

point is well taken and, in policy, this group, as Jack pointed out, has no hesitance in spending money wisely. This is a wise expenditure of money, Jule. (Laughter)

DR. BOGER: There are two points that I have to make, and then I am going to turn the floor over to Dr. Linn, who, with Miss Painter, has prepared some diagrammatic charts describing our sample. I think these will help you to see what we are doing a little better with what results we have at present.

Number one, on training. We have trained our people here, initially, for the pre-testing. Some of the pre-testing was done by people from here who in several cases, we took out to the field. The nature of our region, though, almost precludes this. We very quickly saw that we would be using up an inordinate amount of our budget in travel, so there had to be a more efficient way to do the testing and, we believed, a better way also.

It soon became clear that our various sample centers were not far from good colleges. For example, Anadarko is not very far from the University of Oklahoma, and so forth. So we have worked through other universities, and have found adequately trained personnel in all these areas. Since that time, we have involved these people all the way from some on-the-spot coordination down to actual gathering of data.

DR. PIERCE-JONES: And we didn't involve them by mail.

DR. BOGER: No. No.

DR. PIERCE-JONES: You people actually went and saw them--

DR. BOGER: That's right.

DR. PIERCE-JONES: --and selected them.

DR. BOGER: We have used only two observers in our ORF evaluation. Our feeling here was the fewer observers we used, the higher the reliability would be. We thought that this was perhaps the best way. Our observers were trained here at our December training conference on the ORF, which Dr. Caldwell led very ably for us. Following that, however, we did some further training using some of our video-tape records.

I have to throw in a plug for this because I think--that, perhaps, video tape procedures may enable us to capitalize more on

some of the ongoing activities within the classrooms. This can perhaps be built in as we go along. This would help us all in gaining important data about what takes place in a Head Start classroom.

DR. ZIGLER: Are you familiar with the work that Jacob Cohn has been doing?

DR. BOGER: Yes.

DR. ZIGLER: In this direction?

DR. BOGER: Right.

DR. ZIGLER: That lends itself to exactly what you are saying. I think we should do more.

DR. BOGER: In fact, I am meeting this week with Dr. Cohn. We are working on getting some work going in this area in attempting to further evaluate the ORF. In some of our pilot work, I have been most impressed with the limited impact that video taping seems to have on a classroom activity. We went into classrooms, with cameras, with a small Ampex tape recorder, and with a small bit of equipment, twenty minutes before the class. In effect, we explained to the teacher that, "we just want to take a little tape." And before she could really become very anxious about it, we had hung a mike around her neck, and we were involved and so were the children. I'd be filming here and the kids would be operating at their own concerns in the same room.

DR. ZIGLER: I would like to make a suggestion, Ed, on this. If we are going to get involved in this kind of thing, I would heartedly recommend bringing Cohn into Washington for a day or two. His work is splendid. And many of the dimensions that he has now made measurable and so forth and so on, are not too far afield from some of the dimensions in your instruments, John. And he would be a wonderful resource for this kind of activity.

DR. PIERCE-JONES: We have been approaching this video tape matter from a training point of view to date and have started some conversations with people at these other centers. But I don't know Cohn.

DR. OSBORN: Cohn would be good because he has some very definite opinions on its use in training.

DR. ZIGLER: You have to mention "dimension-wise".

DR. OSBORN: Yes.

DR. ZIGLER: Because you don't have to just start from scratch and pick out what you are going to measure, or look at. He's got that well along the line. And it's really very exciting.

DR. BOGER: I just wanted to bring the matter in, because it is particularly close to me.

I will now turn the floor over to Dr. Linn who will give you some idea of these charts. And then Dr. Cunningham has some brief remarks to make.

THE EVALUATION STUDY SAMPLE

Emma Lou Linn

The University of Texas

These charts are simply a breakdown of each one of our centers by class, and sex, and age, ethnic distribution, Stanford-Binet scores and Pre-school Inventory scores as well as by the language used in testing. With our Indian sample we did use English to test each of the children. Probably the most unique thing about this group (Indian children) was its relative passivity. We have not encountered this phenomenon in any other group of children.

DR. OSBORN: The what?

DR. LINN: The passivity. To get a response from them, you had to work very hard.

DR. NIEMEYER: I don't know--I am a school man. I don't know anything about research so I don't know what preschool raw scores are.

DR. LINN: This is Caldwell's Pre-School Inventory. And at this point, the scores have not been interpreted, because we are now in the process of interpreting and card punching.

DR. PIERCE-JONES: It's quite intriguing, I think, to note from this that the means of the several classes are very close.

DR. LINN: Five classes.

MR. CARLTON: I was asking at that point of time if we know what a normal mean would be for the Pre-School Inventory?

DR. LINN: It depends on the specific age of that child.

MR. CARLTON: I see.

DR. LINN: We have the median age for each class and each center.

DR. ZIGLER: Is that the Caldwell? Is that what you said?

ANADARKO, OKLAHOMA

SMALL COMMUNITY SAMPLE POPULATION 6,300

CLASS	NUMBER OF SUBJECTS	SEX DISTRIBUTION	AGE DISTRIBUTION 3 MONTH INTERVALS	ETHNIC DISTRIBUTION	STANFORD BINET IQS	PRE SCHOOL RAW SCORES	LANGUAGE USED IN TESTING
1	10	M 5 F 5	4-6 1 4-9 1 5-0 3 5-3 1	INDIAN 3 NEGRO 5 ANGLO 2	HIGH 123 LOW 56 MEAN 88.5	HIGH 69 LOW 21 MEAN 50.3	ENGLISH 10
2	10	M 5 F 5	5-0 1 5-3 4 5-6 1	INDIAN 2 NEGRO 5 ANGLO 2 MIXED 1	HIGH 98 LOW 74 MEAN 82.9	HIGH 65 LOW 27 MEAN 45.9	ENGLISH 10
3	10	M 5 F 5	5-0 2 5-3 2 5-6 2	INDIAN 3 NEGRO 4 ANGLO 3	HIGH 102 LOW 65 MEAN 84.1	HIGH 74 LOW 33 MEAN 48.4	ENGLISH 10
4	10	M 5 F 5	4-9 2 5-0 1 5-3 1	INDIAN 4 NEGRO 3 ANGLO 2 MIXED 1	HIGH 121 LOW 64 MEAN 89.7	HIGH 68 LOW 24 MEAN 49.4	ENGLISH 10
5	10	M 5 F 5	4-9 3 5-0 1 5-3 1	INDIAN 2 NEGRO 4 ANGLO 3 MIXED 1	HIGH 102 LOW 87 MEAN 95.7	HIGH 74 LOW 42 MEAN 57.0	ENGLISH 10
TOTALS	50	M 25 F 25	4-6 1 4-9 6 5-0 8 5-3 9	INDIAN 14 NEGRO 21 ANGLO 12 MIXED 3	HIGH 123 LOW 56 MEAN 88.2	HIGH 74 LOW 21 MEAN 50.2	ENGLISH 50

AUSTIN, TEXAS
METROPOLITAN SAMPLE POPULATION 186,500

CLASS	NUMBER OF SUBJECTS	SEX DISTRIBUTION	AGE DISTRIBUTION 3 MONTH INTERVALS	ETHNIC DISTRIBUTION	STANFORD BINET IQS	PRE SCHOOL RAW SCORES	LANGUAGE USED IN TESTING
1	10	M 5 F 5	5-9 1- 6-0 9	NEGRO 8 LATIN 2	HIGH 121 LOW 60 MEAN 84.2	HIGH 69 LOW 38 MEAN 53.5	ENGLISH 10
2	10	M 5 F 5	5-9 1 6-0 0 6-3 9	NEGRO 9 LATIN 1	HIGH 93 LOW 59 MEAN 77.9	HIGH 75 LOW 40 MEAN 52.1	ENGLISH 10
3	10	M 5 F 5	5-6 2 5-9 3 6-0 5	NEGRO 10	HIGH 106 LOW 70 MEAN 88.4	HIGH 78 LOW 46 MEAN 58.5	ENGLISH 10
4	10	M 5 F 5	5-3 1 5-6 2 5-9 1	NEGRO 10	HIGH 103 LOW 53 MEAN 85.0	HIGH 69 LOW 18 MEAN 54.0	ENGLISH 10
5	10	M 7 F 3	5-3 1 5-6 4 5-9 0	NEGRO 10	HIGH 103 LOW 72 MEAN 91.2	HIGH 88 LOW 51 MEAN 66.0	ENGLISH 10
TOTALS	50	M 27 F 23	5-3 2 5-6 8 5-9 6	NEGRO 47 LATIN 3	HIGH 121 LOW 53 MEAN 85.3	HIGH 88 LOW 18 MEAN 56.8	ENGLISH 50

CARLSBAD, NEW MEXICO
SMALL CITY SAMPLE POPULATION 22,000

CLASS	NUMBER OF SUBJECTS	SEX DISTRIBUTION	AGE DISTRIBUTION 3 MONTH INTERVALS	ETHNIC DISTRIBUTION	STANFORD BINET IQS	PRE SCHOOL RAW SCORES	LANGUAGE USED IN TESTING
1	10	M 5 F 5	4-9 1 5-0 4 5-3 2	NEGRO 0 ANGLO 2 LATIN 7 MIXED 1	HIGH 107 LOW 73 MEAN 89.5	HIGH 74 LOW 41 MEAN 57.7	ENGLISH 3 SPANISH 7
2	10	M 5 F 5	4-9 2 5-0 2 5-3 2	NEGRO 1 ANGLO 1 LATIN 8	HIGH 96 LOW 50 MEAN 81.2	HIGH 66 LOW 42 MEAN 52.9	ENGLISH 2 SPANISH 8
3	10	M 5 F 5	4-9 1 5-0 1 5-3 3	NEGRO 1 ANGLO 1 LATIN 8	HIGH 85 LOW 64 MEAN 74.9	HIGH 70 LOW 41 MEAN 54.8	ENGLISH 2 SPANISH 8
4	10	M 5 F 5	5-0 1 5-3 3 5-6 5	NEGRO 0 ANGLO 2 LATIN 8	HIGH 129 LOW 66 MEAN 81.5	HIGH 71 LOW 32 MEAN 50.5	ENGLISH 2 SPANISH 8
5	10	M 4 F 6	5-0 2 5-3 3 5-6 3	NEGRO 1 ANGLO 3 LATIN 6	HIGH 102 LOW 66 MEAN 87.5	HIGH 82 LOW 42 MEAN 60.8	ENGLISH 4 SPANISH 6
TOTALS	50	M 24 F 26	4-9 4 5-0 10 5-3 13	NEGRO 3 ANGLO 9 LATIN 37 MIXED 1	HIGH 129 LOW 50 MEAN 82.9	HIGH 82 LOW 32 MEAN 55.3	ENGLISH 13 SPANISH 37

SALT LAKE CITY, UTAH

METROPOLITAN SAMPLE POPULATION 189,454

CLASS	NUMBER OF SUBJECTS	SEX DISTRIBUTION	AGE DISTRIBUTION 3 MONTH INTERVALS	ETHNIC DISTRIBUTION	STANFORD BINET IQS	PRE SCHOOL RAW SCORES	LANGUAGE USED IN TESTING
1	10	M 5 F 5	4-3 2 4-9 2 5-0 2	ANGLO 10	HIGH 131 LOW 63 MEAN 99.2	HIGH 72 LOW 26 MEAN 55.1	ENGLISH 10
2	10	M 6 F 4	4-3 2 4-6 2 4-9 1	ANGLO 10	HIGH 112 LOW 86 MEAN 100.5	HIGH 76 LOW 29 MEAN 57.6	ENGLISH 10
3	10	M 5 F 5	4-3 1 4-6 4 4-9 1	ANGLO 10	HIGH 118 LOW 88 MEAN 99.6	HIGH 62 LOW 29 MEAN 50.8	ENGLISH 10
4	10	M 7 F 3	4-3 1 4-6 2 4-9 1	ANGLO 8 NEGRO 2	HIGH 135 LOW 72 MEAN 93.3	HIGH 74 LOW 20 MEAN 45.5	ENGLISH 10
5	10	M 5 F 5	4-3 1 4-6 2 4-9 5	ANGLO 6 LATIN 4	HIGH 98 LOW 73 MEAN 88.2	HIGH 47 LOW 21 MEAN 34.4	ENGLISH 10
TOTALS	50	M 28 F 22	4-3 7 4-6 10 4-9 10 5-0 8	ANGLO 44 LATIN 4 NEGRO 2	HIGH 135 LOW 63 MEAN 96.2	HIGH 76 LOW 20 MEAN 48.7	ENGLISH 50

**SALT RIVER, ARIZONA
PIMA-MARICOPA INDIAN RESERVATION**

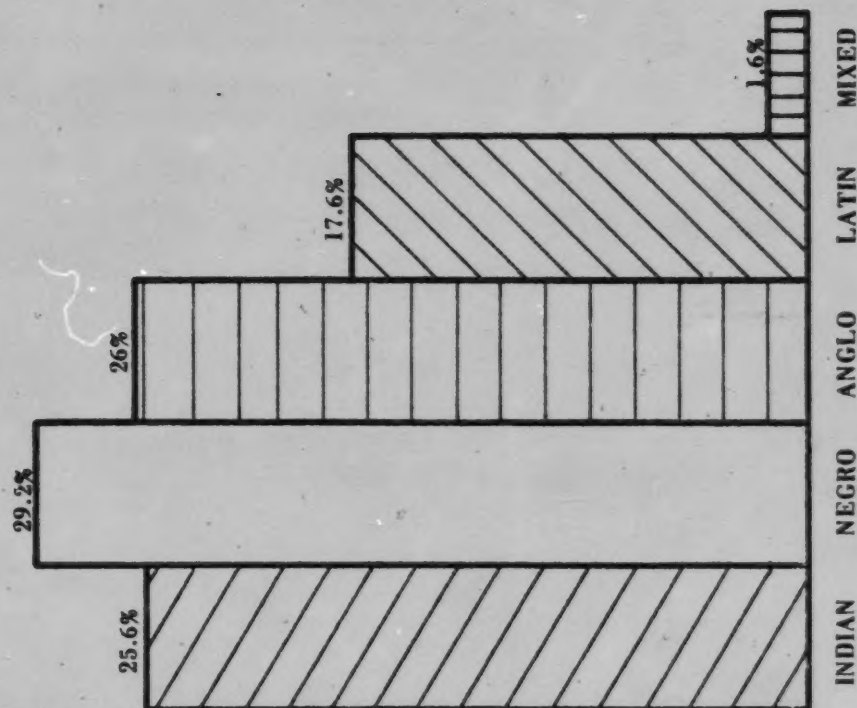
RURAL SAMPLE POPULATION 1,500

CLASS	NUMBER OF SUBJECTS	SEX DISTRIBUTION	AGE DISTRIBUTION 3 MONTH INTERVALS	ETHNIC DISTRIBUTION	STANFORD BINET IQS	PRE SCHOOL RAW SCORES	LANGUAGE USED IN TESTING
1	10	M 5 F 5	5-0 2 5-3 3 5-6 4	INDIAN 10	HIGH 110 LOW 72 MEAN 84.7	HIGH 68 LOW 29 MEAN 47.3	ENGLISH 10
2	10	M 4 F 6	5-0 4 5-3 1 5-6 3	INDIAN 10	HIGH 103 LOW 65 MEAN 86.1	HIGH 71 LOW 16 MEAN 46.8	ENGLISH 10
3	10	M 5 F 5	4-9 1 5-0 1 5-3 5	INDIAN 10	HIGH 123 LOW 74 MEAN 87.7	HIGH 64 LOW 38 MEAN 50.9	ENGLISH 10
4	10	M 3 F 7	5-0 2 5-3 3 5-6 2	INDIAN 10	HIGH 102 LOW 64 MEAN 82.2	HIGH 62 LOW 23 MEAN 47.1	ENGLISH 10
5	10	M 5 F 5	5-0 1 5-3 2 5-6 5	INDIAN 10	HIGH 102 LOW 55 MEAN 82.5	HIGH 68 LOW 24 MEAN 47.4	ENGLISH 10
AM-PM	50	M 22 F 28	4-9 1 5-0 10 5-3 14	INDIAN 50	HIGH 123 LOW 55 MEAN 84.6	HIGH 71 LOW 16 MEAN 47.9	ENGLISH 50
TOTALS							

ETHNICITY

ETHNIC PERCENTAGES OF TOTAL SAMPLE

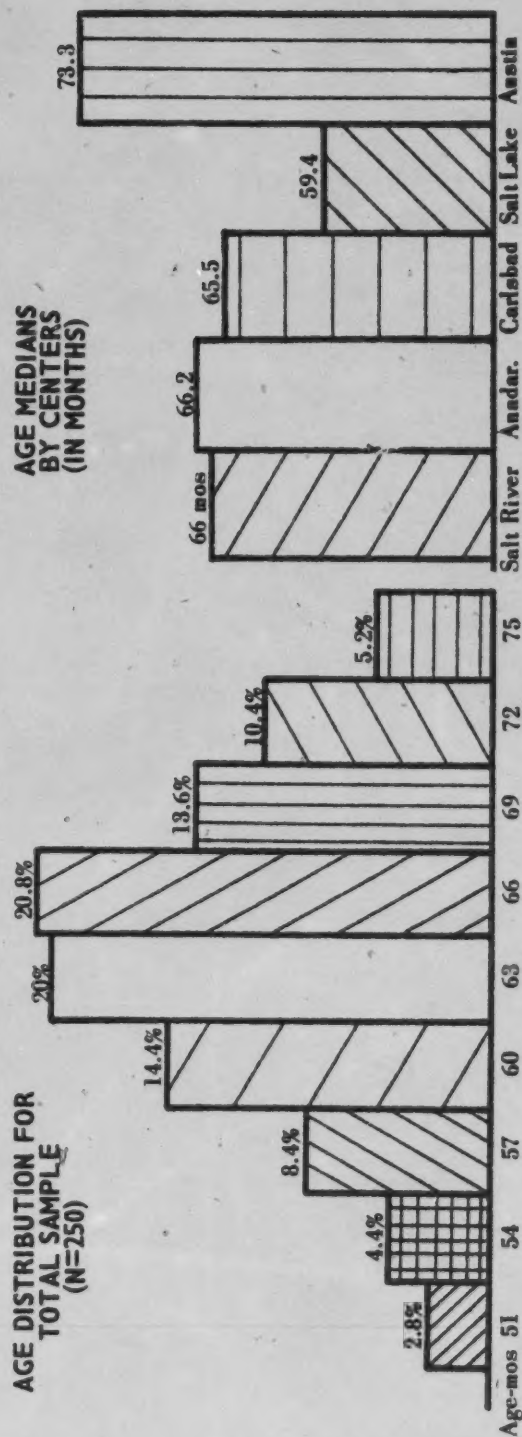
CENTER	INDIAN	NEGRO	ANGLO	LATIN	MIXED
Salt River, Arizona	50				
Anadarko, Oklahoma	14	21	12		3
Carlsbad, New Mexico		3	9	37	1
Salt Lake City, Utah		2	44	4	
Austin, Texas		47		3	
TOTALS	64	73	65	44	4



DISTRIBUTION OF AGES BY CENTERS 3 MONTH INTERVALS

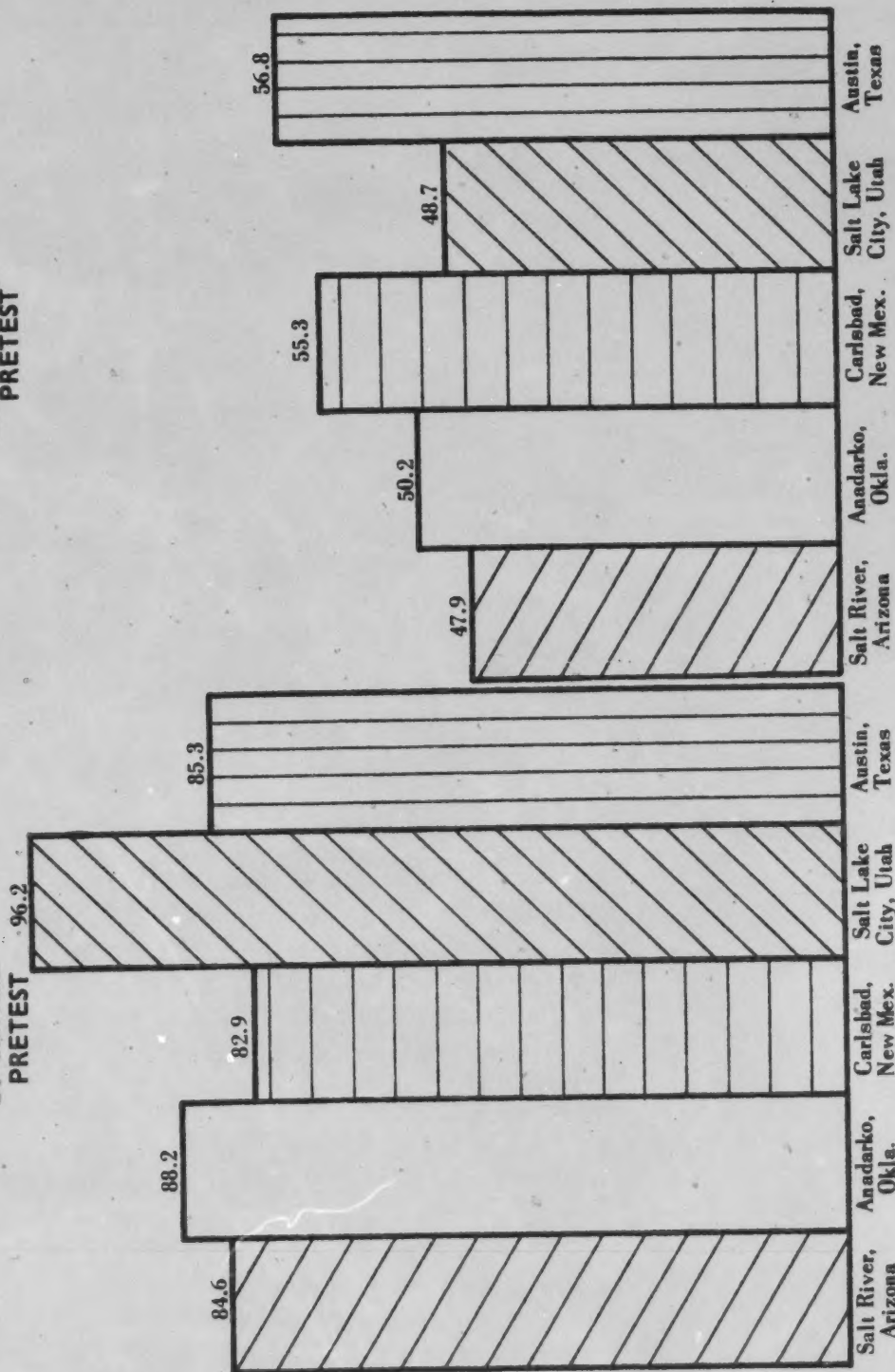
CENTER	51mos	54	57	60	63	66	69	72	75
Salt River	1		1	10	14	15	9	1	
Anadarko		1	6	8	9	12	11	3	
Carlsbad			4	10	13	16	7		
Salt Lake City	7	10	10	8	12	1	1	1	
Austin					2	8	6	21	13
TOTALS	7	11	21	36	50	52	34	26	13

AGE DISTRIBUTION FOR TOTAL SAMPLE (N=250)



**PRE SCHOOL INVENTORY
MEAN SCORES (RAW)
BY CENTERS
PRETEST**

**STANFORD-BINET
MEAN IQ SCORES
BY CENTERS
PRETEST**



DR. LINN: Yes.

DR. ZIGLER: Has that thing ever been put into appropriate numbers so it makes any sense?

DR. PIERCE-JONES: Do you mean, do we have norms?

DR. LINN: Yes, sir.

DR. PIERCE-JONES: For the Caldwell, yes, I think we do have norms.

DR. ZIGLER: Is there a developmental structure or something that we could--

MR. HOOD: Her original instrument has been revised now. It now contains about eighty-five items.

DR. PIERCE-JONES: That's the ETS form, isn't it?

MR. HOOD: Right.

DR. PIERCE-JONES: We have another revision. (Laughter)

MR. HOOD: And incidentally, the University of Texas is, we at ETS feel, superior to the ETS revision. (Laughter) We do have, well, we have a revision, too, by the way. We are sure it will be superior. (Laughter) We are going to have to harden that thing up if it's going to be continually used and get some relevant statistics. Bettye Caldwell revised this test and did send us some norms last February for the '65 group. And then she sent us a lengthy technical report.

DR. BOGER: The norms have increased again.

MR. SUGARMAN: Do you see any correlation between scores on the Caldwell and the Stanford-Binet?

DR. LINN: I have a chart on that, yes. This is the Anadarko (Oklahoma) sample.

DR. PIERCE-JONES: Here, you have both Negroes and Anglos?

DR. ZIGLER: Have you spotted any of this variance? It ranges from eighty-two to ninety-five. That's thirteen points. Is there any big thing that jumps out of the data at you on this?

DR. LINN: Not at this point, unless Linda has looked at it today.

DR. ZIGLER: This looks like an ethnic distribution. They look pretty comparable, don't they?

DR. LINN: They are comparable.

DR. ZIGLER: You have very small groups.

DR. PIERCE-JONES: You have small numbers in each classroom. You have five class rooms per center, you see. We took, you'll recall, as nearly as possible, ten children per class, half the median age, and half below, half boys and half girls.

DR. LINN: This is the Carlsbad sample where we had to test thirty-seven of the children in Spanish. These children there were very easy to test, and had very good cooperation. The IQs, you'll notice, are still running in the eighties.

DR. PIERCE-JONES: Above the average?

DR. LINN: Yes. This is the Austin sample which was predominantly Negro. And again, we have the IQ range--and a mean score again in the eighties.

DR. PIERCE-JONES: Somewhere along the way, it's going to be important to look at some of these rather high IQs of some of these children and at their futures. There are plenty of bright kids popping up.

DR. LINN: This is the Salt Lake City group right here. And in all of the studies in Salt Lake, the mean jumps up into the nineties.

DR. COOKE: Your highest are much higher!

DR. LINN: Yes. With the exception of two classes. And these two classes were integrated classes. A number of children here were classed as Anglo children, but we suspect they were not. They had Latin-American surnames and a characteristic accent, but they and their parents claimed to be Anglo. So we had no other choice but to treat them as such.

DR. GORDON: Are you talking about groups four and five?

DR. LINN: Yes. Right here.

DR. OSBORN: Does this mean that you had no question about groups one, two and three?

DR. LINN: No question in this group. Linda?

MISS PAINTER: No, they were totally Anglo.

DR. OSBORN: But four and five--

MISS PAINTER: Four or five, I would suspect the great majority were not Anglo.

DR. NIEMEYER: Since there were five children omitted from the top three, were any of these--in other words, four and five were the only ones integrated?

MISS PAINTER: No. Two of these classes were completely Anglo.

DR. NIEMEYER: Yes.

MISS PAINTER: These classes were completely Anglo and a third one, I think had about three students out of twenty that were. We also had a special group of Spanish children here that I--

DR. LINN: We had a few that we were not including in the national sample, who claimed not to be Mexican. We tried to differentiate between Mexican-American and Spanish-American. When we checked into the backgrounds we found that, indeed, they were Mexican-American but that they did not claim it.

DR. NIEMEYER: These were children with Spanish surnames?

DR. LINN: Yes.

MISS PAINTER: These absolutely refused to say that they were Mexicans or of Mexican descent. Either they said, you know, where was your great grandfather from and they would say, "well, we don't know." And then you would ask one of his neighbors, and they would say, "Well, we know. Because they say all the time."

DR. NIEMEYER: The four and five groups, are the income levels the same as one, two and three?

DR. LINN: Yes.

DR. ZIGLER: What is this? Do you have any hunch on this? Is the Stanford-Binet a big city test or something?

DR. LINN: I have talked with Dr. DeVaney who has worked with this group and it may be that a hunch about religion has some merit, because it seems as though, in the Mormon Church, they start very young with community education programs and stress academics regardless of the social-economic status of the family. This may be what this is.

MR. CARLTON: Are the Mormons using this program?

DR. LINN: Head Start?

MR. CARLTON: Yes.

DR. LINN: Yes, sir. Many of them are. But, you see, many of these children have already been exposed to community activity in the church.

DR. ZIGLER: You know, this would be the kind of thing our research shows. That if you retest the kids and give them an optimizing test and try to put out the intellect they have, they will go up something like ten points. It would be very interesting to do a design like this where you start out with different means. All this means, that if you retested all those other kids with my kind of optimizing test, they would be like these kids. If you gave them an optimizing test, they would look like middle class kids. We don't get that kind of kids. I don't know if they are smarter or if their early social experience has made them. They give you a better standard test on the first go round.

DR. OSBORN: Well, the trouble with Binet is with what you are getting from the four, five and six year olds. So darned much of it is verbal. So many of those items are verbal items.

DR. LINN: That is true.

DR. OSBORN: I think you really have a problem with the Binet in the first place.

MR. CARLTON: What's your point?

DR. OSBORN: There are so many verbal items that we are really playing into groups one, two and three, where you are getting more verbal contact in community groups than you are with groups four and

five, who may not be getting that. Or with your Latin-Americans who may not be approaching it the same way.

DR. COOKE: Yes. It may be the best way to show improvement, however, in the low groups.

DR. OSBORN: Oh, yes. Sure. Sure it does. But I am not sure that's the point either.

MR. CARLTON: But it turns out that the verbalizing are the ones who are most sensitive to these motivation factors, too.

DR. OSBORN: Yes.

MR. CARLTON: You know, it's there where you have this interchange.

DR. OSBORN: That may be the reason for your difference in opinion.

DR. ZIGLER: There are a lot of questions in this thing.

DR. PIERCE-JONES: I think some of that difference will wash out.

DR. LINN: Now, then, these are the IQ means by centers.

DR. PIERCE-JONES: By communities?

DR. LINN: Yes. This, again, is the Salt Lake City group. There is one important thing to notice. This is the youngest group we had. Now then--

DR. GORDON: The Salt Lake City is the youngest group?

DR. LINN: The median age is 4.9. Whereas at Salt River, Anadarko, and Carlsbad, it is about 5.3. And in Austin, it's 6.1, the median age. So this may be a function of age.

DR. COOKE: You really can't compare the two sides of that chart because the right side hasn't been adjusted for age.

DR. LINN: No. And you see this right here? This test goes only up to the six year level. And the Austin kids, fifty per cent of them were over six years old.

DR. OSBORN: The Salt Lake City kids were lower because they were younger?

DR. LINN: Yes.

DR. OSBORN: Do you have absolute ranging this way? In other words, forty-eight is your youngest group and fifty-six eight is your oldest group. About half of the other three distributed in a range?

DR. LINN: No. No. These range, oh, approximately the same age.

DR. OSBORN: Which three?

DR. LINN: Salt River, Anadarko and Carlsbad.

DR. COOKE: Carlsbad had the same age as Salt River. Is that right?

DR. LINN: Approximately. I am not sure. It's on the next chart.

DR. COOKE: They had a lot lower Stanford-Binet, but higher other--

MISS PAINTER: I have the adjusted scores considering ages if you just want to hear them. For Salt River, it's, these are age percentile means: 44.5 For Anadarko, it's 46.0. And for Carlsbad, it's 59.0.

DR. COOKE: Just the opposite of the other tests.

DR. LINN: Yes.

DR. BROOKS: The two ranges.

DR. NIEMEYER: Is the difference between the spread of forty-five to sixty-five significant on the scale?

DR. LINN: Well, I tell you we have to--I think it's going to be.

DR. OSBORN: That one probably will. But if you only have fifty kids, I don't think fifty is--

DR. BOGER: Of course, this is very interesting in terms of the differences in the programs. Very definitely, differences in them.

DR. ZIGLER: Is this the correlation we are supposed to use, from these charts to a set of rules that we will see later?

DR. LINN: The IQ is what I was talking about with reference to the Salt Lake City group in comparison--

DR. BOGER: One other difference which is very interesting between Salt River and Carlsbad, is that these two groups began with language communication difficulties. We used Spanish forms so that we could probably assume a maximal communication with the Carlsbad group. The Indian children in Salt River were bilingual but the extent of this bilingualism was, I am sure, highly variable and consequently I am sure that communication differences were involved here.

DR. LINN: We could not find out to what extent bilingualism existed.

MR. SUGARMAN: This is the point of entry into the program.

DR. LINN: Right. Well, it's the point we tested them.

MR. SUGARMAN: Yes. Yes.

DR. ZIGLER: Why are they so old in Austin. When does school begin? (Laughter)

DR. PIERCE-JONES: There is no kindergarten.

DR. LINN: There is no public kindergarten. There is also no public kindergarten in Salt River and no public kindergarten in Carlsbad.

DR. ZIGLER: What age do they start the first grade here? Don't they start at six?

DR. LINN: At six years.

DR. PIERCE-JONES: They have to be six years old by the time school starts in the fall. By the first of September. Head Start probably picked up a lot of kids who missed school entry age by a month.

MR. CARLTON: And this picked up a bunch of kids that became of age after the first.

DR. LINN: These programs started in the fall. Salt Lake City has kindergartens and so does the particular district in which we tested does have public kindergarten.

DR. OSBORN: I think all of Salt Lake has kindergarten.

DR. LINN: Yes.

DR. ZIGLER: That's really interesting. The things we don't consider until we see charts, like this. You know, we are always talking about four year olds and five year olds. But it turns out we are really dealing with, you know, a lot of kids who are pushing seven, and undeveloped mentally. These are quite different children.

DR. OSBORN: Fifty-three per cent of the kids in America today aren't in kindergarten. There are no kindergartens.

DR. ZIGLER: All right. The point I am trying to make though, is how are we going to get people like Judy (Miss Cauman) here to be concerned about these educational programs? The Head Start program is appropriate for three year olds and four year olds.

MISS CAUMAN: I am concerned. But we talk about the children and they could be three, four, five or six and from all different backgrounds and we aren't doing enough.

DR. BROOKS: Do you have ethnic comparisons in terms of funds?

DR. LINN: Well, I'll tell you the nearest thing we have to that is the--we are going to do it, of course, but at the time we made this up, we did not have all the materials available. But right here, of course, these are all Indian children. These are, with the exception of six, all Anglo children and these were the, with the exception of about six, all Negro children.

DR. OSBORN: Did you do any analysis or anything like this in terms of some rather hard physical data?

DR. LINN: We have not yet begun our analysis. In fact, we only finished pre-testing recently.

DR. OSBORN: But you have no X-rays of any kind to get, you know, physical development quotients?

DR. PIERCE-JONES: We have not done any of that.

DR. BROOKS: I am not sure you ought to either, if you want my opinion. I think your radiation hazards are greater than your gain.

DR. PIERCE-JONES: We don't have a qualified technician.

DR. BROOKS: You have a wide, wide variance.

DR. OSBORN: I was interested in seeing if they were getting ethnic differences in bones.

DR. BROOKS: I don't think spraying some X-rays around is probably the best way to do it. (Laughter) I think they are dangerous.

DR. LINN: Do you have anything on this?

MISS PAINTER: No.

DR. ZIGLER: There is a lot of ethnic data on this thing that is available already.

DR. LINN: Do you have anything on this?

MISS PAINTER: No.

DR. ZIGLER: There is a lot of ethnic data on this thing that is available already.

DR. LINN: We also have one other thing that is interesting. The Indian children were the most passive children I have ever worked with personally. And in Salt Lake City, these children were the most aggressive. You can tell the difference between noise levels of the rooms, and things like that.

DR. NIEMEYER: John, on this passivity level, you might want to note the customs and check other things--

DR. PIERCE-JONES: Yes, I think we should. There are lots of possibilities, I think, for cross-hatching groups and comparing them.

DR. LINN: That's it.

DR. PIERCE-JONES: Why don't we then let Dr. Cunningham have a word.

DR. CUNNINGHAM: You want to do that before lunch? It's twelve o'clock.

DR. PIERCE-JONES: We will prolong this session.

DR. CUNNINGHAM: All right.

DR. PIERCE-JONES: We have been concerned with problems of data analysis and even data recording. And we haven't been supplied by Washington with any format to use. So we decided to go ahead on our own. And Dr. Cunningham has developed some data punching formats and has some ideas he may present. Dr. Cunningham.

STATISTICAL STRATEGIES FOR EVALUATION STUDIES

Grover Cunningham, Jr.
The University of Texas

In front of you, you will find a sheet which is headed "Statistical Design." In it I have outlined what I want to talk about. We have already talked this morning about the two kinds of questions that we want to seek answers to. There has been a great deal of work done on this first question in terms of what statistically significant changes are effected in Head Start children. I think that generally the consensus is that we are, through this program, achieving this in dimensions that are meaningful and beyond the usual maturation of the child. But those questions are still answerable in terms of the data that we are collecting. And I think that, while we may not spend a great deal of time on it, they are certainly there for analysis and we should continue to look at the question.

The second question which has been the primary area of focus is in terms of: What kinds of programs? What sorts of teacher variable? What sorts of effect on these children?

The next section of this outline, and please, feel free to stop me whenever you want as you have been doing, is in terms of statistical approaches to analyzing these questions. And there are two methods which are suggested here as alternatives to analyzing these questions.

One approach is the analysis of variance approach and the other is in terms of multiple linear regression. I've attempted to sort of generalize the two approaches here.

Under the analysis of variance model, the first one is in terms of classifying your children in terms of groups and then analyzing the data in terms of the pre-measure and the post measure. The comparable model in multiple linear regression terms is a prediction model in which you attempt to predict a score based on certain vectors which you know the information concerning. In other words, I have generalized this here. What you are attempting to predict is a score. Whether it is pre or post, you put all these into one vector of scores.

On the other side of the equation, you develop some vectors which are classification vectors, in which you assign a value of one,

if this particular score is from one group; a zero otherwise. This could go down to make it comparable to the analysis of variance model.

A "W" is a vector, in this equation, is a vector where a one designates a post score and a zero a pre-score.

"X" is a vector, and I've illustrated it there in each case, designating that the one is a score from a member of group 1, and a zero otherwise.

And then "Y" in turn is from Group 2. You enter in that vector a one or a zero otherwise.

MR. CARLTON: Isn't that an error in typing?

DR. CUNNINGHAM: Pardon me?

MR. CARLTON: Isn't that an error in typing? You have zero, one, one, zero.

DR. CUNNINGHAM: Well, you mean zero, one, one, zero?

MR. CARLTON: What does that mean?

DR. CUNNINGHAM: Well, that indicates that score number in your criteria, the zero indicates that that score is not from a member of group 2. Your "one" in that vector indicates that this score is from a member of the second group. In other words, you can have more than one "one" in a particular group. The "a", "b", "c" and "g" equal unknown weights to be solved for in a particular fashion.

DR. ZIGLER: Why use a multiple linear regression model at all? What are you trying to build? A maximum prediction equation?

DR. CUNNINGHAM: That's right.

DR. ZIGLER: Why no multiple analysis of variance?

DR. CUNNINGHAM: These are comparable here, that is, the two are interchangeable and are able to analyze the data in the same way. I think that multiple regression is a more economical method of attempting to attack our second question, but, certainly, traditional analysis of variance would be appropriate here also.

DR. ZIGLER: What I am saying is that I think it's "way too early in the game" for selecting maximal predictors. What we really want to find out is what variables are operative, especially those in the interactions which will come out of your classic analysis of variance designs. These regression weights probably won't be very much better than the reliability of your predictor instruments, and we don't have a very good handle on those right now. The result is that any statement you make about how much variance you are picking up with particular measures is probably a very rough estimate. It doesn't move you much beyond an analysis of variance that tells you only that a given variable is operating. Once we know that a variable is operative, then let's build the kinds of measures that make the multiple regression analysis sensible.

MR. SCHUHMACHER: I'd like to make a comment here. Analysis of variance is essentially equivalent to multiple regression, and you can answer the same sorts of questions with it. It also gives you the freedom to use variables, as predictors, that cannot be forced into the one-zero system. For instance, in other Head Start research we did, one of the things we wanted to investigate was how much we knew about the children, on several variables, influence the differences in pre and post test criterion scores. We were using these difference scores as criteria. One of the obvious things that we have found, one which is going to help in predicting the changes, is knowing where the child started from in the first place. That, as you know, is the child's initial score. Children who start out with very high scores tend to improve somewhat less than children who started out with low scores.

Now, this isn't saying, of course, that the scores can be forced into a dichotomous classification.

DR. ZIGLER: But you can do it at levels very easily. You have continuous levels of intelligence. You don't have to classify dichotomously. I can understand this design in terms of the computer, but I think that it could be misleading to the extent that you think you are really explaining a certain amount of variance. I always associate this kind of thing with being far enough along the line that you don't have to worry about all kinds of inbreeding and unreliability effects. And another thing: the assessment that you get of how much variance is taken out is not very good, at this stage of the game. Do you think it is?

MR. SCHUHMACHER: Well, I think we may be conceiving of this in somewhat different ways. The intention is not necessarily to build a predictor model so much as it is to conduct a test of statistical significance.

DR. ZIGLER: Okay, but, obviously, this is also what you would do if we were going to build a maximum predictor system. As long as you are saying to me, "Well, I want to find out what variables are operative, but it's something I don't need in a maximum predictor," fine!

DR. PIERCE-JONES: And then, of course, we must develop experimental manipulations of the statistically influential factors.

DR. ZIGLER: Okay, fine!

DR. CUNNINGHAM: What is available to put into the generalized model includes, of course, the center, the teacher, age, sex, ethnicity, and so on, used to classify people according to these various groups. We have, of course, pre- and post measures on the Behavior Inventory, Caldwell's Pre-School Test and the Stanford-Binet.

One might talk briefly and generally about strengths, weaknesses, and plans for improving inferences that can be drawn from the data at hand, or with particular reference to specific criteria and problems, for example, the Behavior Inventory. In the latter case, for example, you have the special problem of consistency of measures across-observers.

One technique that we plan to exploit is a factor matching approach, to match the factors across the rater populations, and to build factor scales for the Behavior Inventory.

DR. PIERCE-JONES: And, in addition (and for Ed particularly), to see whether different groups of raters seem to utilize the same set of construct dimensions in rating children. This is where the factor matching comes in.

DR. ZIGLER: Are you checking for differences in factor structure?

DR. PIERCE-JONES: In a sense, yes.

DR. ZIGLER: Has anybody ever devised a very good factor matching test?

DR. PIERCE-JONES: It depends on what one means by "good." Ahmavaara, in Finland, developed one technique about ten years or so ago. More recently Henry Kaiser's approach has appeared. We are using Kaiser's approach.

DR. ZIGLER: Some people have already sent me the results of their factor analyses; there are some differences. One analysis showed three factors, one had four factors.

DR. CUNNINGHAM: We are getting three factors from our summer 1966 data - three that account for some fifty-five per cent of the variance.

DR. ZIGLER: Are you looking at different ages? Have you looked at that?

DR. PIERCE-JONES: We have not as yet. We do have one study going that hasn't been reported in any of our progress reports, however. In that study we are using Shaffer's instrument with the Behavior Inventory.

DR. ZIGLER: Well, I got a lot of material from Shaffer.

DR. PIERCE-JONES: Good! Then we should find substantial correspondence between that device and yours, Ed.

DR. GORDON: You might want to contact him (Shaffer) if you feel fairly comfortable about your data. He has expressed an interest in looking at some of the Behavior Inventory material, but the data we have from '65 are so questionable, that we haven't sent that to him. If yours is in pretty good shape--

DR. CUNNINGHAM: Okay. We think that, probably, the consistency across-observers (with reference to the other instruments that we used to collect pre and post data) is pretty good.

On the Caldwell Pre-School Test, Dr. Zigler, did I hear you mention something about some work that gives the benefits of some sort of control group, such that the changes that occur on the test can be ruled out as maturational effects?

DR. ZIGLER: Gee, I don't know, but by the nature of the instrument, I think it would be highly sensitive to maturation effects. You would almost have to have controls unless you were to develop some kind of developmental quotient, and stick the CA in it.

DR. PIERCE-JONES: Well, we have scored the Pre-School in various ways that haven't been widely used elsewhere. For example, we picked out the items that showed definite developmental increments, and we have called it a "developmental increments scale." We have been scoring for that.

DR. ZIGLER: That makes good sense. The Caldwell is much too long.

DR. PIERCE-JONES: Well, we've got it down to seventy-five items. ETS has eighty items. Our reliabilities are of the order of .75-.95 on five subscales. It's passable, technically.

DR. ZIGLER: One thing I would like to see the Centers get together on is this: if we are going to keep using this instrument (which I still don't understand, frankly) we are going to have to put it in some kind of rational system. You know, really do something with the thing so that it is used the same way in different places, and see whether it's picking up any variance independently of the Stanford-Binet.

DR. CUNNINGHAM: To move on to another question: What kind of programs and teachers have what effects on what kinds of Head Start children? Here I find the multiple regression approach most appealing, personally. Rather than classifying on perhaps eight different dimensions, one can take the actual raw scores, for instance from the ORF, use your post-test score as your criterion, then build your multiple linear regression model to take account of all the relevant factors which are being measured and which might influence the post-test score of the child. In effect you are then asking which of the hypothesized predictors are accounting for significant amounts of the variance in the post-test (or criterion) score.

DR. ZIGLER: Maybe I am missing something--why use the post-test score? Why not use the pre-post difference score?

DR. CUNNINGHAM: Well, we will be doing it in both ways. We intend to run it both ways. In other words, our data include difference scores, the post-test score, and the pre-test scores.

MR. CARLTON: How complex are your breakdowns of the different characteristics - for instance, parental characteristics?

DR. CUNNINGHAM: Well, the parental interviews yield some data and the teachers also gives some information about parental participation in the program. This comes from two sources; from a parent interview in which people from our center actually go out and interview parents concerning their participation and involvement in Head Start.

MR. CARLTON: Do you get things like economic breakdowns, whether or not both parents are living at home, what kind of job they have, number of children?

DR. PIERCE-JONES: Yes. And we have proposed an addendum to the interview form, one which one of our Social Workers is working on.

DR. COOKE: Who scores those and how do you score them?

MISS PAINTER: I imagine I will, but we haven't yet. This work is scheduled for next month.

DR. COOKE: You don't have parent characteristics collected yet?

DR. LINN: No, we haven't collected the data.

DR. BOGER: We are presently in the process of training the interviewers.

DR. COOKE: You would like to do this before the kids get into it, some of the parent characteristics, I would think.

DR. PIERCE-JONES: Yes.

MR. CARLTON: This is one area where we have occasionally had problems: that is, all of us have had problems in the area of sensitivity to the impact on the public of asking certain kinds of questions. So we've had to move out a lot of very pertinent material as being just been the type of thing that you can't do in a government program. I suspect that we are going to have to omit more, because our public affairs people have looked at some of these questionnaires and are saying, "wait."

DR. ZIGLER: Somebody has to speak to this business. There is more involved in Head Start than image. One can go along with concern, but there are ways around some of these problems. I don't think that the last court of appeal should be the public affairs.

DR. COOKE: I agree completely.

MR. CARLTON: It's not the last court of appeals, of course. But they have an effect on it.

DR. COOKE: We are supposed to have a scientific group (of our own) a group that I have been pushing, that would make decisions about whether or not these various interviews were invasions of privacy. This goes on in every good institution also.

DR. ZIGLER: Right. One thing that you can do with these kinds of parent interviews, and that is that you voluntarily pay the parents for their time. But, my God, I don't think that the scientist, or people concerned with science, should simply give in on all of these things. I think we have to do some education, and we have to find some other techniques. We also have to see that our ethics are as good as we can make them. I am really very concerned if Head Start is not collecting needed and pertinent data for the development of good Head Start centers, simply because of problems of this kind rather than trying to work them out.

MR. CARLTON: The questions that we really have had problems on, so far, are in the areas of sex and religion.

DR. PIERCE-JONES: Things such as appear on the Edwards Personal Preference Schedule?

MR. CARLTON: Yes.

DR. GORDON: We have tremendous pressure from the Bureau of the Budget now, to keep this decision-making to itself. There was a moment, however, a brief period, when they felt they might let representatives of the research community make recommendations.

DR. OSBORN: This is what I thought was happening. I thought you were setting up a review board.

DR. GORDON: That was in process, but it has been reversed for the moment. I don't know quite where to say the issue is right now.

DR. PIERCE-JONES: This afternoon, we would like to give you an overview of the individual studies we are pursuing. Thus you will see the considerable diversity of the things we are doing. Some of these studies may speak to some of the questions that are arising in the group now.

MR. CARLTON: Somewhere during the day could somebody give us a picture of the steps, the total number of steps, that are part of the evaluation process. You know, where you stand on it. How they come in sequence, like your parent interviews - the ones you are going to do in April? That would be helpful to me in understanding the entire process.

DR. PIERCE-JONES: This certainly can be arranged. So why don't we take a luncheon break and come back?

(A forty-five minute luncheon break was taken at 12:25 o'clock.)

AFTER LUNCH

DR. PIERCE-JONES: Now to the quarterly reports.

DR. COOKE: Do you have to get out a quarterly report?

DR. PIERCE-JONES: We have got a lot of stuff going on and it's burdensome to have to prepare these lengthy reports.

DR. COOKE: No other agency I know of has to prepare quarterly reports.

DR. ZIGLER: This is contract, however.

DR. COOKE: Do you think this is a good idea?

DR. PIERCE-JONES: I would like to complain a little about it and I am sure Bob and Emma Lou and Grover and others on our staff would also like to register some protest.

DR. COOKE: Is this going to be a continuing problem?

DR. GORDON: The contract at OEO? The only thing needed for OEO is an assertion from me that I am satisfied that the work is progressing. The kind of report that has been made so far isn't needed for that purpose. We can relieve that.

But IED, which is the management agency for these Centers, feels that it needs a record that is adequate enough for them to guarantee to us that John isn't using the money in some other way; that the work that is supposed to be done is progressing in a certain way. I suspect we can get it reduced somewhat from the present level, but unless their legal counsel is more flexible than he has been in the past, I suspect he is going to want to have enough to make them (IED) feel completely certain.

DR. COOKE: I am completely against the quarterly report.

DR. PIERCE-JONES: As a basis for preparing the second quarterly report, we were given a list of eleven questions to speak to. It took a good deal of time and paper work to deal with them properly. In fact we have a regular "assembly line", report production facility.

DR. GORDON: You mean just for reports?

DR. PIERCE-JONES: Well, not just for reports, but for surveys, and the like.

DR. ZIGLER: More important than your quarters, John, is really the complaints that you may have. How is this arrangement with IED? How do you feel about this? I mean, here's Head Start, and here is IED, and here's you. And there are demands made on you from sundry levels. How is that working in your estimation?

DR. PIERCE-JONES: Bob and Grover should check me on this. I feel, personally, that there's been no trouble at all. Have you noticed any on a contractual basis or anything else that's very irksome, Grover?

DR. CUNNINGHAM: No.

DR. BOGER: My only complaint from down here where you have not seen the report issue, is that it establishes a "middle man situation," which at times may duplicate things.

DR. ZIGLER: Ed (Dr. Gordon), how do you think things have been? Are you satisfied?

DR. GORDON: You know, Ed, that requires a long discussion. It has presented problems for me which I don't think we ought to get into deeply today. I'd rather turn your questions back to the Texas people and ask them if there are alternative arrangements that they think might be even better. Since I suspect the problems that I have encountered have not necessarily been problems for them.

DR. PIERCE-JONES: It seems to me, now that the Centers have been established around the country, that we could readily contract directly with OEO.

DR. GORDON: What kind of benefits have you derived from their involvement?

DR. PIERCE-JONES: I should say the frequent trips of Mr. Josephson to Washington to negotiate with your contracts officer.

DR. NIEMEYER: Is that a benefit?

DR. PIERCE-JONES: Well, perhaps so.

DR. GORDON: It makes it easier for you to operate, the fact that there is a management man negotiating with our contract office?

DR. PIERCE-JONES: Yes. I think it has been so for the whole group of Center Directors, though I surely can't speak for them.

DR. GORDON: Are there any professional or substantive services that you have had so far?

DR. PIERCE-JONES: No, but perhaps we haven't needed them.

DR. COOKE: Have they said, for example, that the study described on page 46, say, ought to be rearranged in such and such a way or do they just weigh the document? If it's over a certain number of pounds, do they approve? Do you know?

DR. PIERCE-JONES: Well, at the risk of being a little more candid than is perhaps discreet, I received a letter, recently, acknowledging receipt of our second progress report. The individual who wrote it said something like, "While I have not had an opportunity to examine it in detail, it looks as if it meets our requirements."

DR. ZIGLER: Let me push Ed Gordon's question a little further. IED has put some child development types on their staff. Have these people, these child development consultants, helped you in any way?

DR. PIERCE-JONES: We have had no substantive contact with them so far. The contacts which we have had with IED have been mainly on contractual matters.

DR. ZIGLER: It seems like a lot of horsepower to do a management kind of job like that.

DR. PIERCE-JONES: I think it may increase the overhead, perhaps.

DR. ZIGLER: Thank you.

DR. OSBORN: I think that the fact that you get your money may be because they are doing something that is not immediately apparent.

DR. PIERCE-JONES: Yes, I agree. I want to make an announcement. We have arranged a garden party for this evening from five 'til about eight o'clock. We will provide transportation, since "Shank's pony" is not usually up to a ten mile trip. I hope, therefore, that we can close out the review of our research program by four o'clock.

I will take minimum time, now, to give an overview of our independent research program as distinguished from our evaluation component. And, in this connection, you should know that the first decision I made was that it didn't matter whether a research person had a Ph.D. or not. It did matter whether he had ideas, and whether or not he seemed to be able to execute them - ideas that, in my judgment and in the judgment of a small group of us, were worth pursuing.

We received research proposals, with budgets, from about two dozen University of Texas people. Finally, we decided that we would go with some eleven of them.

DR. ZIGLER: How did you get word out that you were searching for such proposals?

DR. PIERCE-JONES: Well, Dave (Dr. Holmes), how did you find out?

DR. HOLMES: I think I heard from Dr. Stolz.

DR. PIERCE-JONES: Dr. Stolz, did you hear it from me?

DR. STOLTZ: Yes.

DR. ZIGLER: There was no official statement, let's say, to the Texas Psychological Association?

DR. PIERCE-JONES: No, this was strictly intramural. And frankly, a lot of people had been my students. Others were faculty people whom I knew either directly or indirectly.

MR. SUGARMAN: You didn't set forth any areas that you were particularly interested in? It was open ended?

DR. PIERCE-JONES: I don't really think so. Grover, will you speak to this?

DR. CUNNINGHAM: Well, in informal communication, we used the term "early child development." This was our emphasis, and, of course, we furnished a summary of the research guidelines set out in our contract with IED.

MR. SUGARMAN: This is right.

DR. NIEMEYER: And did this go to various departments, divisions, and groups with the University?

DR. PIERCE-JONES: We were in contact with the School of Social Work, the Department of Sociology, the Departments of Educational Psychology, Psychology, Marketing, and the Child Development Clinic at the Medical School in Galveston where obviously I knew Bill. On balance, however, we have to view this as a personal net of some kind.

At any rate, we have a number of studies going, and all of them seem significant to someone. Some seem more significant to me than do others, but I have reached the point where I feel I can't always pass on other people's ideas with any arrogant confidence.

DR. ZIGLER: You selected how many out of how many, John?

DR. PIERCE-JONES: Eleven, the eleven that we have here, out of about twenty.

DR. ZIGLER: And how did you do that? Who made the decision?

DR. PIERCE-JONES: Cunningham, and Boger and Linn and I met together and made the decisions.

DR. ZIGLER: As a kind of research evaluation group, you then went over these proposals?

DR. PIERCE-JONES: That's right. We knew we were going to have to be responsible in the end for the reporting and we made these decisions in the light of the funds we had available, or thought we had available.

The studies in progress range across a broad spectrum. If I had to suggest that we have a center of gravity in the program, it would probably have to be in the area of linguistic behavior, the study of language behavior. There are at least four studies of language behavior in this program. We also have some perceptual studies. One study has to do with "neurological correlates", at least, if not with basic neurology. Another one, Mr. Spellman's, is concerned with ethnic group differences in the parameters of stimulus preference and choice behavior. And we have some other pieces of work concerned with personality, broadly defined - Mrs. McDaniel's work, for example.

Finally, because the Child Welfare League of America came to me a couple of months ago and asked me to be on it, we will take part on a Panel at the National Conference on Social Welfare at the end of May. We will deal with the impact of OEO programs on Children's Services. We have not had any really hard data by which to make any

judgments of these matters, so Miss Ann Maurer, of our School of Social Work, and I have now undertaken a national survey of local (and some county) welfare agencies charged with directing OEO programs. We have developed a questionnaire covering that and have sent it out. The sample covers about twenty-five per cent of local agencies and about ten per cent of county welfare agencies nationwide.

In setting things up for this afternoon, we have asked each of the individual investigators to talk with you briefly. You will notice there are eleven people listed. We have tried to organize them with two considerations in mind. One: when people would have to leave us this afternoon. Two: in terms of the substantive questions our people are addressing.

Since Dr. Holmes and Dr. Stoltz have to leave us, we have put them first. Dr. Holmes is a new member of the Department of Psychology faculty - David.

EFFICIENCY OF NEURAL CONDUCTIVITY

David S. Holmes

The University of Texas

Thank you, Dr. Pierce-Jones. As the title of my project suggests, I am interested in what I like to call, very globally, "Efficiency of Neural Conductivity". This is a very global sort of thing. I am not sure that anyone has really got a handle on exactly what is going on, in this area, but some of us have some ideas about it. Let me give you a little background.

Background

Some years ago, Krech, Resenzweig and Bennett, at the University of California, initiated very interesting work dealing with brain chemistry in the white rat in relation to differences in performance - learning, adjusting to new environments, and so on. Their analyses suggested that acetylcholine, the level of acetylcholine, was a most important variable. As time has gone on, they have changed their position as to exactly what the mechanism is, but it deals with transmission of the nervous impulse.

Many people have talked about applying this work to human beings, but it is a rather difficult undertaking, since it involves sacrificing the organism in order to do a chemical analysis of the brain.

In about 1964, or, 1963, at the Pennsylvania Psychiatric Institute, Leonard Rubin, came out with a study of the speed of pupillary contraction in psychotic and neurotic patients. And some neuro-physiologists have suggested that the speed of pupillary contraction is related to the level of acetylcholine, an inferential measure. The data here are quite substantial. It's complicated, but most of us tend to agree that this is, in fact, a good evaluation of what the pupillary response is measuring, that is, just the speed with which the pupil contracts. Differences have been found in the way patients performed, in symptomatology, and so on.

DR. ZIGLER: He found differences in response to what?

DR. HOLMES: To light. Just straight light. This should

be distinguished, of course, from the work of the people at Chicago, who show people pictures and who then note dilation and contraction.

The procedure we use involves hitting the pupil with a constant, intense light. Then we photograph the pupil as it contracts, and determine the per cent of contraction which occurs during the first two seconds of stimulation. You get a substantial variation across people, even though the stimulus is the same in every case.

I became interested in this phenomenon. I thought that it should be related to other sorts of problem solving. So a year or so ago, I did a study which showed that the speed of pupillary contraction was significantly related to certain personality traits, particularly introversion-extroversion, both as measured by self report and by ratings. It was also significantly related to speed of problem solving.

These results reinforced me, and so I did more work. If you are interested in that particular study, I have some reprints here, and you can go into it more thoroughly. Following that, I related the pupillary reaction to other forms of problem solving and to concept formation. This was strictly a pilot study. It involved only thirty subjects. The data approached significance in the predicted direction, even though it was only a pilot study.

Still another study which I have done in this area related the speed of pupillary contraction to personality types. It replicated the original study, and it also related the phenomenon of interest to the duration of figural after effect, which we can relate neural conductivity.

What I am doing, then, is taking a measure which we all think ties into neural conductivity and trying to triangulate in on it, if you will. When I heard about the possibility of getting data on a lot of young children who were, in a sense, going through important developmental phases, and on whom we are going to have a lot measures of intellectual development, confidence, perceptions, social development, and the like, I thought this would be an interesting pool of data in which, to be perfectly honest with you, to do a little "pot boiling." I have some specific hypotheses, but they are very tentative.

Data collection in this study is fast and cheap. It takes about one minute per subject to collect the data. It's taken about five months to get the apparatus built, but we have now overcome the

difficulty we experienced. The data can be collected in a normal room. I don't need a light-tight room for this work.

Once the pupillary photographs are taken (this is done with a sixteen millimeter motion picture camera), it's very simple to get percentage scores and so on, and you can do various transformations.

Now, I am going to look at how my data relate to children's performance on the various tests which are being administered in connection with the evaluation enterprise. I am not collecting any other data.

Let's see. I have hit the background.

DR. COOKE: How is neural conductivity, measured in this way, correlated with neural conductivity measured in the conventional way?

DR. HOLMES: I am not aware of an actual study relating these two variables. What this is measuring is not the speed at which the impulse travels down the nerve, but the efficiency with which it jumps the synapse.

DR. COOKE: Right. Somebody ought to try to relate the two, I would think. There may very well be differences and similarities. The pupillary response measure was used for a time for certain groups of sick children, as you know with children with cystic fibrosis. There were supposed to be big differences. I wonder what nutrition and other factors may have to do with response. I would first, like to find out whether there is a relation between pupillary response and other measures. If this is the case, then we need seriously to look into it. I think it might be easier than trying to study a lot of other variables and then performance measures. It's an intermediate response, an easy response to collect data on.

DR. ZIGLER: What measures do you think it ought to be related to?

DR. HOLMES: Well, I rather suspect it should be related to social measures, social performance.

DR. ZIGLER: Why? Do you really think that AC has that much to do with social measures?

DR. HOLMES: Well, yes, in terms of impulsivity. This is essentially what we are measuring. This is the thing which, three times, I've been able to relate to pupillary response quite nicely.

DR. COOKE: Of course, there are all sorts of things modifying this pupil besides nerve impulse, for example if you have some character who is all upset with the test situation.

DR. ZIGLER: This is the argument I have had with Smith. I hope I don't have the same argument with you sometime in the future.

DR. COOKE: This is subcortical, isn't it? Or is my neuro-anatomy wrong?

DR. HOLMES: This is one of the interesting leaps of faith which the neurologists have suggested we make.

DR. COOKE: That is a big one.

DR. HOLMES: The data, because of the difficulties of getting at these levels in humans, has to be related - the pupillary response to peripheral levels - and one has to decide then whether he thinks peripheral levels are related to central levels.

Now, I was rather skeptical of this, but I am becoming less skeptical of it as I see the pupillary response being nicely related to things like figural aftermovement effect. It's hard to imagine how peripheral levels of acetylcholine or whatever it is I am tapping - I talk about acetylcholine, but I am using this very loosely - it's hard to imagine how my peripheral levels are affecting the duration of figural after effect.

DR. ZIGLER: I like that better than talking about social things, because, after all is said and done, we have to take the kinds of things that Hunt and others have been saying seriously. You know, his stuff on the situational determinants of the kind of responses that we are going to hear about being picked up at Head Start Centers. I would bet that these would be at least related to the pupillary response.

Getting down to the level of figural after effects, this is the kind of measure that such people as Herman Smith have been using in relation to notions of neural sluggishness accompanying IQ. His theorizing overlaps yours to the extent that Head Start children are lower IQ children.

The thing is that his work has "come a cropper," because of the same kinds of things that may influence your work. You don't get a pure product in your measure; a kid may have trouble with your instructions or anything of that sort. It's the same kind of

thing that Bob was saying, and it could influence your predictor measure.

DR. HOLMES: Okay. First of all, you are forgetting that in getting the figural after effect data, we are using college students, with whom I don't think I have instructional problems. The data came out nice, neat, and orderly, exactly as predicted, with college students at Northwestern University. I don't really think I had too much difficulty in terms of instructions with them.

DR. ZIGLER: That runs out of most of my argument.

DR. HOLMES: This is essentially why I was studying this, because I dislike the research on figural after effect in brain damage. Let's face it, you know, they are different kinds of people.

DR. ZIGLER: What was this relationship? What was the magnitude of the relationship.

DR. HOLMES: Well, let's see. We were talking about significance beyond--

DR. ZIGLER: I am not interested in significance.

DR. HOLMES: I am trying to arrive back. The work was done on an analysis of errors breaking subjects up into high, medium, low; and fast, medium, slow, pupillary contraction groups, then using the duration of their figural after effect.

DR. OSBORNE: I just mentioned, if we are going through all the people, I figured out nine minutes a person. We are already in thirty minutes.

DR. COOKE: We probably ought to let people go through things.

DR. CARLTON: Some of the things that we may be looking for as a planning committee are not the adequacies or inadequacies of the particular study, but how this relates to our own objectives.

DR. OSBORN: And what it contributes to Head Start.

DR. PIERCE-JONES: I think I will be a little bit heavy handed here, if you don't mind, and operate as a dictator or as some students have said, "A pulpit centered teacher." Did you get through, Dave?

DR. HOLMES: Yes.

DR. ZIGLER: I'll come down some other time, John. (Laughter)

DR. PIERCE-JONES: You are welcome any time.

DR. ZIGLER: I am going to be here next summer.

DR. PIERCE-JONES: I think we will move on. Dr. Walter Stolz is a faculty member in the Psychology Department. He comes from The University of Wisconsin to Texas by way of Harvard's Center for Cognitive Studies.

DR. OSBORN: Wally, will you tell us the page your material is on? I lucked into this one.

DR. STOLZ: Oh, it must be about thirty. Yes, it is thirty.

DR. PIERCE-JONES: That is exactly where it is.

THE SOCIALIZATION OF DIALECT

Walter S. Stolz

The University of Texas

I shall try to say fairly quickly what is going on. The study is really composed of two quite different things.

One of these is pretty much a straight dialect study of a very interesting group of Central Texans who might be called "rural hillbillies." They object to the name "hillbilly", I think, but these are the people who are called "cedar-choppers" in this area. Their history is somewhat shrouded in rumor. One rumor is that they were, originally, a band of people from the Ozarks, and they migrated down here. The fact is that these are lower class people, very deprived people who have been here a long time. They are not migrant workers; they are barely workers at all.

What they do is, they chop cedar. I don't know whether you know the kind of growth that covers the hills up north and west of here. But it's largely cedar and liveoak and they chop this for firewood and for fence poles and so forth. And they hunt and they fish and they live off the land. They trap. And they are as close as five or ten miles outside of Austin.

They are very isolated, I guess. They don't live in clans, really, but it appears that they are within a stable group with interests unto itself. There are just a few families.

At any rate, they often live in dirt floored, tin roofed shacks. Their children, sometimes come up through the high school, but they don't wear shoes at all. Even in the winter here, where the weather gets fairly cold, they come into school without shoes. They are just very, very isolated and deprived.

I had never done a study like this before, but I was interested in some work that had been done under the general topic of social linguistics. William Labov has done some very nice work in New York City, "Social Stratification of Language in New York City." There have been some others - scattered studies.

The importance of these has to do with a very usual sort of notion that a person's dialect changes with respect to his reference

group, if you want to be sociological about it, and I am really not. But the more he feels himself a member of a group, the more typical of the group his language will be.

So at least superficially, our study is testing just that sort of thing, not with pre-school kids but with early primary grade children. We have taken first, second and third graders, three groups. A control group of middle class, suburban Austin children, which is the population from which virtually all Central Texas first, second and third grade teachers come from. Our experimental group consists of particularly deprived children out in a particularly deprived area called Leander. It's about twenty miles north of here.

We are getting several measures for the children. We are getting three measures of socialization, of how well they fit into the school. One of them is the standard teacher rating, which I am getting because it is easy to get. The other two are, I think, a little more interesting. We are devising them ourselves and having fun. One we have picked out in conference with teachers, can be called four values. This involves four or five values that they try to instill in lower class children. The whole idea is that these children, the Leander kids, just don't see a middle class person up close until they come into school. Their culture, the world they live in, is very, very different. So what we are doing is to "pick on" cleanliness, inhibition of aggression (I guess we dropped patriotism; that didn't pan out), manners (a certain degree of manners), and general attitude towards school.

We have composed some questions as well as we could and have revised the questionnaire with standard kinds of things. We have pre-tested and so on. We are fairly sure that our questions have something to do with the socialization of the kids in the school situation. They are taking on the values and so on of the school.

And the other measure that we are using is a little more interesting, but, in a sense, more tenuous. We are trying to use semantic differentials with these children. We have worked out a verbal and oral form of semantic differential using four-point scales, and a child only has to make judgments such as, you know, is mother good or bad. Then, is she very good or very bad. It seemingly works well. We don't have any good reliability statistics on it yet. We are just getting the right data now, but from looking at what's in, it's fairly good.

We are getting SD measures on about ten concepts, "my mother," "my teacher," "father," "school," "how I'd like to be," "my best friend," and a few other things we picked with forethought. And we are also having the teachers rate a "model child" on the same thing, telling us how they would essentially most like a child to react.

And then, of course, an obvious measure of socialization is merely to take a teachers rating--how they would like the child to fill out the form if the child was most like they would like to be and take a D square measure of the difference between a child's rating of mother or father or school and so on. We take a difference-distance measure between them, teachers rating and the child's rating.

Well, those are the socialization measures. Two of them I have listed in order of conventionality. In the first quarterly report, I listed some precedents for socialization being measured in terms of semantic differentials. The idea was that it would be fun to try to do this at a child's level.

Then we are getting language samples from these same children. The idea, regarding socialization, in the language part is to collect samples of the children's language from using thematic apperception test pictures, a few of these and a few Children's Apperception Tests. We use them just to elicit language. And we are getting, oh anywhere from five to ten or fifteen minutes worth of language, depending on the child, in a diadic situation, talking to a person who is probably identified with the school in the child's mind.

Secondly, we are trying to get a little different "style" tapped with these children. We have set up, with the teacher's consent and actual blessing, a "show and tell" situation in front of the class. Here the child is describing one or two of the pictures or telling the story, spinning a yarn in front of the kids. That is quite a different thing for these kids. These discourses are being tape recorded.

This brings in another point which is even more interesting. We are identifying linguistic variables, oppositions between middle class Central Texas dialect and cedar-chopper's dialect, on two levels. We were hoping to do this on three levels, but it looks like that won't work out. It is going to have to be on two levels. One is the phonological, the other is the semantic level.

Our progress now is that we've got our pretesting done and we are just now in full swing getting our actual data. We have

interviewed about thirty-five people in a structured manner to get dialect samples from adults - anybody, really, over ten years old. We have a stratified sample of cedar shopper types between the ages of fourteen and seventeen and unknown.

To illustrate some of our variables: The lower class, the cedar choppers, tend to make a rather obvious error in agreement between singular and plural markers on verbs. Like, "they was born in Austin." That was an example I picked up on a recent tape. There were two errors of course. Again, many seem to omit subjects completely in sentences. A very common error is to put "that" in for "who" when introducing a relative clause. Like "lots of them that lives here." Then there are lots of the double negatives that, I guess, are fairly widespread in every lower class dialect. They also show a very systematic omission of auxiliaries. We are not quite sure what we have yet in this, but some of the examples are: "I done some farming," "They just gone to ruin," "There is never a day pass," and so forth.

Some of the phonological variables that we are working on are very obvious and clear. In lower class cedar chopper types, there is a very common reduction of "ing" to "in." And this doesn't seem to be the case in middle class Central Texanese.

There are also some differences in collides, as you probably know.

DR. OSBORN: Excuse me. I am being very impolite. At least for me, I get the drift. And I apologize. But I am worried about time at the other end.

DR. STOLZ: Then this is the way we are going. This is the kind of thing we are doing. If you have questions, I'd be glad to answer them as well as I can. Our status is that we know what our variables are. We know what we are measuring now. We have done a good deal of our testing.

I might add one admonition that is holding true and that has to do with the time limit on the money. This would be a project on which I would love to have half as many people working twice as long rather than twice as many people working half as long. It's going to be a real bind to get things done by the end of June. And I have a feeling that quality is suffering because we don't have time to pre-test properly.

DR. PIERCE-JONES: We will be able to continue exploring this area, won't we?

DR. STOLZ: We can't plan to get results and get them out.

DR. PIERCE-JONES: I neglected to tell the members of the planning committee and the Washington group here that one of the things we said to people in soliciting proposals was that we didn't know how the funds would be the second year and that, frankly, we wanted things that could be wrapped up in the summer to give some output so that we wouldn't have to count on longer term operation.

MR. CARLTON: Dr. Stolz, if your hypothesis is correct, what do you see as the significance of that confirmation?

DR. STOLZ: Well, the significance is that, first of all, it's interesting psychologically to know that, in some sense, kids are paying attention to this sort of thing at this age. It's pretty obvious with adults that this sort of thing goes on. It's already been confirmed by William Labov two or three times by an unpublished study that was run at Yale that came over with Yale undergraduates. The whole idea of what kids pay attention to at an early age, I think is, then, a psychologically very interesting question. The focus that I am even more interested in, as I did mention, was that we are also looking for lexical levels - differences between dialects but these are difficult to get.

I am more interested in the kind of change that goes on. Does a child, when he shifts, tend to shift his dialect. Does he shift all at once? Do children essentially learn middle class English bilinguistically? My feeling is that they do. In fact, I have a child, a five year old, who gives me intuition, because he talks mid-western at home and he talks Texas as soon as he gets out of the house.

DR. PIERCE-JONES: Mine do that, too. And do it very deliberately.

DR. STOLZ: This is the kind of thing that is even more interesting to me as a psychologist, because phonological-syntactic versus lexical things are probably, at least, more dependent on psychological models than we usually suppose and this is rather difficult to show.

DR. PIERCE-JONES: I think another point is that we are backing a method down, really, in some aspects of this study from the college student and the adult level to the younger level.

DR. STOLZ: Yes, step-wise. That is the whole thing. One of the things that could come next, and if this ever gets extended, what I would like to do, for example, is to check if various phonological markers do play a definite role in stereotyping. If you hear somebody say a particular collide on a vowel, you may be absolutely unable to report that that is a sign of lower classism, and yet this affects the rating of a person.

DR. OSBORN: This is what--I forget her name - the gal in Detroit was finding, that Negroes in Detroit were immediately classed.

DR. STOLZ: Again, how far down the line do we have to worry about this or is it a significant factor in the essential socializing of children? What is the kid's attitude, for example, to a middle class accent when he comes into the school? Is he neutral, if he doesn't notice these things except as he comes along in school or notices them unconsciously? If a cedar chopper comes in with the idea in the first grade that a middle class accent sounds bad, it's a bad stereotype. I think that's of significant practical value. These are the kinds of things that we are working up to - the kind of data, dialect data, that we are getting. It may turn out comparable to some stuff that is going on in Detroit.

DR. ZIGLER: Is Keith Osborn asking all these questions that are slowing everything down now? (Laughter)

DR. PIERCE-JONES: All right. Let me cut it off again.

A PERCEPTUAL COMPONENT OF VISUAL-ANALYTIC SKILLS

Edward Gotts

The University of Texas

This report begins on page twenty of your studies booklet (quarterly report).

DR. PIERCE-JONES: Which one? The first one or the second one?

DR. GOTTS: This is the first report. I will begin by noting a series of cognitive style studies by Segal, by Kagan and others that have uncovered a general dimension of "reflection-impulsivity". More investigation in the same area, however, has led to the identification of another aspect of this dimension called analytic skills. Reflection is viewed as a contributor to general cognitive ability. A cross sectional study in groups of children, grades one through four, has shown that response latencies increase and recognition errors decrease with age across time, suggesting that the more time the person, the child, takes to reflect over a possible answer to a visual task, the more likely he is to come up with the correct answers.

Another line of study which is of interest here is by the Polish psychologist, Batska, who has studied the visual identity behavior of children from six months through five years. Through a very ingenious procedure which involves no verbalization, she was able to discover that by age three, children do, in fact, detect the identity of objects they have seen and are able to identify the objects from among other objects. By age five, this habit was very well established, so that children's responses were about 93 per cent correct by age five. These are, however, relatively long exposures when the child has an opportunity to look, first of all, at the object he has identified already for two seconds and then the object which is to be selected from another group for as long as he wishes.

Back to Kagan's work. Kagan, after his cross-sectional studies - after he had detected the importance of these dimensions - went on to a longitudinal study, in which he followed children from kindergarten to second grades. He found a predictive relation between reflection and impulsivity and analytical skills with the development of reading skills. It was possible to predict approximately sixteen per cent of

the variance in reading ability on the basis of scores independently of what was known about the children in terms of verbal abilities or intelligence previously. This is a fairly large prediction in the area of future reading skills.

It might also be noted that the disposition of interest here can readily be related to some of the signs of affective growth which are mentioned in Krathwohl, Bloom and Masia's Taxonomy of Educational Objectives for the Cognitive Domain. We would suspect that being able to reflect over things like this would also be correlated in other types of classroom behaviors that may be readily detected in middle class children and in children who have not had the necessary opportunities to engage in verbal communication. In this sense, reflection-impulsivity may be highly relevant to school readiness.

Up to this point, Kagan's studies have dealt only with the correlational aspects of the relationship. There have been no attempts to modify the behavior. In this study the purpose is to extend this work from a correlation study to a "treatment study" in terms of a curricular type of experience for the children, one that should increase their ability to make visual detections.

First of all, we have developed a pre-testing procedure and have selected the same sample that is used in Austin for evaluation purposes so that we have complete overlap with all the other sample data. The pre-test procedure initially involves going to the classrooms to select objects that are on display commonly in all of them. Thus, instead of coming to the children with some presumably standard set of objects based on our assumptions, we come with objects which are in the room and currently available for their inspection. We have drawn these on, in India ink, on cards that fit into a reading device by an individual child.

In the pre-test, the child has one set of objects that are related to his classroom - objects that are unique to his classroom, and he does a warmup series on these. At this time, we try to establish a threshold at which he recognizes fifty per cent of the objects correctly, and then we try to determine the exposure time at which this occurs.

When we have established this, we then administer a set of objects which are collected from his room and which are common to other rooms also. I might mention that, wishing to control for possible differences in the child's ability to produce verbal responses, we arranged all the testing materials on a table so the child simply had

to point to the correct object to indicate his recognition of what was being displayed at that time. Each child has control over the exposure of the objects, because he knows when it's going to come. In fact, he himself presses the switch that causes the object to be illuminated.

Once we have established that we are nearing the child's threshold we use the set of objects selected from his classroom but which also are in common with the other four classroom in the Austin sample. These are administered to establish a definite threshold, a "pass threshold" and a "fail threshold." This we do by using a descending and an ascending series. If we need to interpolate thresholds on our devices, we can do this within our procedure.

Following this, we have administered a series of tasks in which the child has a card displayed beside him on a table and four objects are placed on cards individually in their respective positions. The child is to pick out from this series the object that he has seen. Among these, we have letters and so on that are placed in various positions. We would suppose, with more familiar objects of the sort being used in the establishment of the thresholds, that the children would experience some gains from their prior experience and from generalization. But these objects with which the children have not had as much familiarity, would constitute more of a test. However, at all time, what the child is to identify is placed beside him on the table. Thus, the child can actually look at the picture he flashes on, can compare to see which it would be, and then can simply point out the object that he has seen.

On these bases, we classify children into two groups. That is, on the basis of their scores, we designate groups above and below the median. Then we randomly select half of these two groups and administer additional treatment to these. The treatment, in this case, is an instructional period.

DR. ZIGLER: Does the error score define the impulsivity?

DR. GOTTS: The error score in this case, defines non-reflective guessing.

DR. ZIGLER: How do you know the problem is a guessing problem and not defective impulsivity?

DR. GOTTS: We establish the threshold, first of all. This is the purpose of doing this in the beginning. This is something

that hasn't been done in Kagan's studies. In Kagan's studies, all children have had to be working in the same settings. But the child's individual threshold was not determined.

DR. ZIGLER: It seems like the better indicator though to slow down the administering of it; give him a longer time and see if he showed improvement rather than this.

DR. GOTTS: You maybe have groups that are quite heterogeneous in respect to your independent variables. This concerns me because there is more than one way of making an error. One defined by your experimental dimension and this you probably wouldn't be interested in.

DR. ZIGLER: There is another study on this that hasn't been published yet in which this person had people take the Stanford-Binet or some sub-test and gave it two ways. One the natural way and another way where he makes the kids slow down. And you get this big improvement score which is the kind of thing, I am sure your experimental manipulation is going on to.

DR. GOTTS: Determining the child's threshold is only a matter of convenience so that when he uses the standard test, he's doing it under conditions of equal difficulty with the other children in terms of his own ability to detect objects at that speed and classification.

DR. ZIGLER: Yes, I see your reasoning. I am just wondering if that still is the most conclusive measure of the dimension.

DR. GOTTS: Let me clarify something else. My interest is not in the cognitive dimension or reflection-impulsivity. As I view this, we are dealing, in our study, with a smaller perceptual component of this. This does not constitute a measure of reflection-impulsivity. I think we discussed this earlier in my conceptualization of this. For instance, we are not attempting to relate this to certain personality things as has been done with reflection-impulsivity.

DR. ZIGLER: What is the dimension here?

DR. GOTTS: It's the perceptual dimension of correct recognition of objects which would be presumably related to the child's ability to recognize the difference between letters that look very similar when he is later presented with regular reading problems.

DR. ZIGLER: My criticism has no relevance, whatsoever. Except I don't know why you muddled up the waters with an earlier reference to impulsivity. I thought that was what you were going on to do.

DR. GOTTS: No. The reason for doing this is simply to point out that since this particular thing has not been studied in relation to a later event in the child's life, we would like to know if there is some relation between this perceptual habit and how he is going to do in school later. These other lines of evidence are indirect kinds of support for the value of conducting this kind of study.

DR. ZIGLER: But to the extent that the other lines of evidence had to do with impulsivity, they are irrelevant.

DR. GOTTS: Okay. But insofar as part of the predicted relation that was established across time was determined by errors scores, then we are talking about the same thing.

DR. ZIGLER: You don't know how much of it, though, in Kagan's work is one way or the other.

DR. GOTTS: Yes, we do.

DR. ZIGLER: You do?

DR. GOTTS: These were analyzed separately. In fact, it turns out that there is a sex differential in relationship to response latency in the error score.

DR. ZIGLER: It was better for girls than boys?

DR. GOTTS: Right.

DR. OSBORN: This is what I was wanting to ask. I know we found that. Have you given any thought to what I thought you were finding in this difference between perception in the two dimensional and three dimensional objects?

DR. GOTTS: Okay. These are all two dimensional objects.

DR. OSBORN: Oh, I see. I didn't understand that.

DR. GOTTS: So essentially, the idea is a very simple one. The idea is that, within a couple of years after they are in the Lead Start class, the children will be facing the task of associating a particular sound with a particular symbol unassisted by color or size or anything else, but simply by form. So the form of an object is a relevant dimension for detecting whether an instance has occurred or has not occurred.

DR. ZIGLER: Has that ability been shown to be related to reading readiness? I am sorry. Has that been shown to be related to reading ability?

DR. GOTTS: It has been shown to be related to reading success.

DR. ZIGLER: That's in your work. But I am not talking about impulsivity.

DR. GOTTS: Error scores have been shown to be related to reading success.

DR. ZIGLER: The reason I asked it that way is that - from North Carolina, has been working with Head Start kids and has come to the conclusion, you know--his is quite a different work from yours. You have to match the sample and all kinds of things according to reading readiness, and he has found it is unrelated to reading ability.

DR. GOTTS: From a common sense approach, let's say, the child is faced with a task. For the first time he is given an abstract symbol which comes to him as a form, a two dimensional form. He is aided by no other dimension here such as color, depth, intensity, or anything else that he might have used to discriminate objects earlier. But we do know the children are able to detect form at this time, from Babska's studies. We therefore expect all of the children to be able to recognize forms. The question is, "How accurately will they do this?" Can we improve their accuracy? Later, when they have to recognize these and read at a very rapid speed and associate a sound with a particular sign and with semantic properties of words grouped with other words, he is being asked to do a whole lot of things at once. Anything that we can do to make one part of his task easier for him and to facilitate his reading at a later time is of value.

So, what we propose to do in terms of a treatment then is to give children additional experiences, to give half above median and half below the median group in error scores, the additional experience of working in an individual session with the device, viewing objects at their lowest success speed, within the classroom in individual sessions. We had hoped to give as many as five or six sessions per child, but I think we are going to get only two or three sessions, and these, of course, may not make any difference.

DR. ZIGLER: It's so artificial. Why should that be the kind of thing you would give a child? You may be training a kid to

be doing things that may interfere with reading later on, simply by teaching him that he has to do things very, very rapidly. What led you to this?

DR. GOTTS: Okay. We have an entirely different paradigm in the instructional period, that is, you don't guess unless you know what you have seen. Instead of having everything assembled on the testing table, the child has to go find the object in the classroom on the later occasion.

We hope, by this, that one of the things that may occur to assist children in improving their object recognition is that they will attend more to objects that are on display in the classroom. One of the differences on the post-test may be due to this sort of attending. We have not controlled for this. It would be worthwhile if we had a pre- and a post measure of how much the children are attending to objects in the classroom, to see the difference between these two groups.

DR. ZIGLER: One final remark, before I'd like to see that kind of effort extended experimentally, I'd like to see just what relationship, what later reading performance of your high error and low error group is. Just what is that relationship. Is it high enough to be worth our concern? Let's take an extreme case. You have a high error group. So you find out the low group and the high group do just as well in their reading later on. Is this possible, given the newness of your techniques? Next, summer, I am going to come in.

DR. PIERCE-JONES: We will have another session.

DR. GOTTS: Please remember that we are not forcing the child to take the task at a fixed setting. We are finding what the child's recognition threshold is.

MR. CARLTON: How will you know when the children are successful in reading?

DR. GOTTS: All we want to do right now, and again, I bought into this with the understanding that this is a "one shot study." And if I had been given any assurance that this was anything else, I could have given much more. All I want to do now is to show an increase in correct recognition for the children who get the treatment over the children who do not have the treatment. That's all we get at this point.

DR. OSBORN: Is it fair to say, if we got you a hundred thousand dollars, you would be happy to investigate this further? (Laughter)

DR. GOTTS: I think that would be too much money.

DR. PIERCE-JONES: At Indiana University, where Dr. Gotts is going in June.

DR. ZIGLER: That seems to me to highlight something of problem of yours. Two of your leading researchers are leaving.

DR. PIERCE-JONES: Well, there may be others coming up the line.

DR. ZIGLER: They may or may not be interested in the same problems.

DR. PIERCE-JONES: Well, we will find some people who are. Nevertheless, it is a problem, this staff continuity matter, and it becomes a special problem here at Texas, because the policy of the University generally is not to keep its own Ph.D.'s around more than one year beyond the doctorate, at least not with faculty rank. If they don't have faculty rank, and are willing to settle for Research Associateships, they can stay until Hell freezes over, of course.

DR. COOKE: Why can't these people get faculty rank?

DR. PIERCE-JONES: It's just a matter of the Regent's policy against inbreeding.

DR. COOKE: Not hiring your graduates?

DR. PIERCE-JONES: Not hiring your own people. I'd be very happy to keep Dr. Gotts here on a lot of scores. Ed, is there anything more? I didn't mean to cut you off.

MR. CARLTON: I can see this is going to be a great dinner tonight. (Laughter)

DR. PIERCE-JONES: I am going to change the order just a little bit. Dr. James Hubbard, Director of Counseling and Guidance in the Austin Schools, is also interested in certain facets of language behavior. And he, too, is doing a study in our program. I will let you, with your poor, limited voice, Jim, talk about your work.

ORAL LANGUAGE DEVELOPMENT STUDY OF HEAD START GRADUATES:

PROCEDURES AND PRELIMINARY RESULTS

James L. Hubbard

Austin, Texas, Independent School District

I wonder if it will be all right if I stand up? I think I can control this voice a little better standing up. If it gets too bad, I will stop. I guess I want to stand up, too, because I have chopped a little too much cedar to be completely comfortable at this gathering. (Laughter) I believe that we are on page twenty-three of the second progress report.

Now, perhaps, one of the first things that I should emphasize here is that what I want to describe is totally embedded in a school situation. I will be concerned with the first and second grades, with that portion of these two grades which has been exposed to Head Start and with another portion which has not.

We are working in a group of schools which, collectively, are those involved in our Title I program. These involve sixteen different campuses. Actually, there are fifteen elementary school units because we have one campus that goes up through the third grade and another from the fourth through the sixth.

MR. CARLTON: Excuse me, I don't want to miss something. Are these pre-school programs?

DR. HUBBARD: No.

MR. CARLTON: School age program? Title I?

DR. HUBBARD: No, it's a school age program, an elementary school program as I used it here. Another characteristic of this setting is that we have, by virtue of the characteristics of the Title I program, some auxiliary personnel who have been useful to us in "setting up" in gathering data, and in controlling the manner in which we go about the whole process. And doing it as nearly as possible in the completely naturalistic setting where the child finds himself every day. We are simply eliciting some information in that setting.

Now, in each of the two grades, we have an N of about thirty Head Starters and Sixty non-Head Starters who come from the same ethnic population, the same background, and the same neighborhoods.

The only difference on the face of it, at least, is that the one group has been exposed to Head Start and the other has not.

In our total Title I project, as originally developed and presented, reading achievement improvement was offered as one of our objectives. Very quickly, of course, we came smack up against the fact that trying to measure reading achievement, particularly in the very early primary grades, is no mean task. The use of the conventional standardized achievement test leaves a great deal to be desired when you are dealing with this kind of population.

So, we started looking for a different approach. We settled, here, upon something that we have called "Oral Language Development," the premise being that the development of oral language precedes both the introduction to reading and the development of any reading facility. We thought that, perhaps differences in oral language development would be of significance to us in any subsequent investigation of reading behavior.

It's in this setting, then, and with this kind of premise, that we got started. In doing so, we have developed one instrument that we are working with and this again has been done, using the people who were to employ the instrument. What we did was to go to our reading supervisors, and through them, to the reading teachers. We solicited from them, lists of items which in their judgment were descriptive of "oral language development." These fell into two broad groups, one group having to do with the "mechanics of language expression" and the other having to do with what we have called the "expressiveness" of the individual, that is, the willingness of the individual to use language to attempt oral communication.

We refined and edited our items, those that were submitted by the teachers and supervisors. We then resubmitted them, and went through the fairly conventional process of distilling an instrument, and we came up with a fifteen item device, to which we attached five-point rating scales to be used to rate a sample of language obtained by a teacher, by a counselor, by a supervisor. We did not specify who should use it, though, in our case, we have used the regular classroom teacher who also teaches reading. In our Title I program, the supplementary teacher is a reading teacher; we have used these two people to rate the same sample of language from the same group of children.

In order to get some control over the kind of samples that were used and over what was rated, we introduced the tape recorder into regular classroom practice. This took a good deal of doing

with a number of the teachers who insisted that the children simply would not talk in front of a tape recorder. I am sure you know that it was really the teacher who was reluctant to have anything to do with the tape recorder, not the children. Once we had gotten this point established, we went through a period where the tape recorder was used in regular classroom operations, with smaller reading groups or in whatever way the teacher chose to handle it. Once this had become an operational feature, the teacher was asked to obtain the sample of oral language that was to be rated.

Right quickly let me remark on the organization of the students before we ever went into this. The teachers had arranged their students into three operating groups for reading, high, middle and low. We accepted this without changing it. Then we asked them, in each class, to get samples from each of these groups, from two students in each of these three groups. This is where we got our total N of 90 in the fifteen units.

This enables us to make a number of what we hope will become meaningful comparisons between the high group and the low group. Because I am feeling rather quickly, let me say that we have just completed the collection of the initial round of data. We will do it again before school is out.

A quick summary of our results at present looks something like this: Oral language development appears to be more homogeneous in its distribution in the Head Start population than in the non-Head Start population. This is true even though we did not, generally, find significant differences in mean scores between the two groups, particularly at the first grade level. We did not find significant differences in the first grade level between either mean item scores or mean total scores. Something that will prove very interesting if it stands up, and it looks rather promising at this point, is that we did find significant differences in mean item scores at the second grade level between the Head Start and the non-Head Start population. Most of these differences, in fact all but one, were items involving expressions, the willingness of the child to participate in oral exchange. Of course, as we have operated Head Start, this was one of the intended objectives. If it stands up, it will be interesting to note that it shows up with a "delayed reaction" appearance rather than from the very beginning. Of course, this makes it suspect to start with; nevertheless, that's what it looks like at the moment.

I think I had better stop at that, because, if I had anything more to say, I don't think you would be able to hear it.

DR. PIERCE-JONES: You won't be able to answer questions, Jim, but may I say that I think that was a very nicely organized presentation. Now, I know what you are doing.

We have a linguist on our staff. A man who has done research with the Peace Corps, one who has published considerable research on non-verbal communication, Mr. Ed Cervenka, who is doing important work for us in bilingualism. It is centered in the early elementary grades for the present, but, again, I have taken the position that Head Start youngsters do get to public school. If we are going to do follow-up in longitudinal studies, we are going to have to be prepared with instruments and methods, so some of our work is deliberately pointed in that direction.

I think Mr. Cervenka can speak for himself, as a linguist, and I will let him do so.

TESTS FOR BILINGUAL CHILDREN

Edward J. Cervenka

The University of Texas

My project is reported on page twenty-five in both project reports. My aim has been to develop tests for measuring both the language proficiency and the adjustment of bilingually educated children. I have samples of the tests I have devised, if any of you want to examine them in detail.

A number of public elementary schools in South Texas are cooperating with me on this project. In these schools, both English and Spanish are used as a medium of instruction. This project was instigated by Dr. Theodore Anderson, Chairman of The Romance Languages Department at The University of Texas, and it grew out of the need, felt by those involved in bilingual education, for suitable tests for measuring language proficiency. It was realized that language tests were needed, because, in the case of South Texas, English was being taught to Spanish speaking children and Spanish to English speaking children in different sorts of ways. These languages are being taught as living languages, as instruments of communication and interaction and less as academic subjects. The goal of these programs is to teach a foreign language to children so that they can actually speak and use it effectively - almost as a substitute for the mother tongue.

One of the claims, or justifications, for the bilingual program is that it has a significant and positive effect on the adjustment of the Spanish-speaking, disadvantaged, child. Casual inspection and observation of a bilingual program usually bears this impression out. However, there is need for an experimental validation of this claim.

Our tests, hopefully, will serve two purposes: they will serve as research instruments, and, eventually, as standardized language proficiency tests in English and Spanish for children. Our general guidelines in test construction held that proficiency tests in English and Spanish should cover, comprehensively, all aspects of the language system for English and Spanish: Pronunciation, grammar, vocabulary and meaning. The proficiency tests would also measure the child's use or control of a language as an instrument of communication and interaction and not merely as an academic, or

intellectual, exercise. This meant, in effect, that the tests should cover primarily oral and aural uses of language--listening and speaking. Still another general guideline was that the proficiency tests should employ local dialects of English and Spanish.

Our tests were developed for use by individuals normally available in school environment. No specialized training ought to be required for their administration; experts are not needed for scoring our tests. No recording apparatus is used. These tests are suitable for use as pre-tests and as post-tests for children from six to nine.

Our measures of "adjustment" are intended to be culturally fair. Any test of adjustment that involves language should be given in the child's native language, specific behavioral characteristics selected for observation should be relatively equivalent in the two cultures involved.

The methodology followed in the development of additional tests was essentially that of Robert Lado. His methods are presented in Language Testing: The Construction and Use of Foreign Language Tests. This methodology follows his two sections on (1) the selection of content items for tests and (2) the selection, or invention, of testing techniques or situations.

Section of the content for the tests, contrast was used. The analysis of English and Spanish was used to pinpoint the problems of a child learning a second language. That is, where English and Spanish differ, it is a problem for a child learning a second language. For example, the "s" and "z" sounds exist in Spanish but carry no functional load. They do not distinguish meaning. So this is a semantic problem for the child, from Spanish to English.

Also in selecting items for curriculum and materials other materials used in the three bilingual programs were examined. Other curricula or programs for teaching English and Spanish in the primary grades were consulted.

MRS. CAUMAN: Excuse me, could you speak a little louder, please?

MR. CERVENKA: Frequency lists were consulted but not heavily relied upon. And also studies of South Texas Spanish were consulted. Teachers were interviewed to check dialect forms of Spanish.

In regard to testing techniques, to the extent possible, group testing techniques were applied. However, this was not always possible. Proficiency tests now consist of twelve subtests, five of which must be administered individually and the others either individually or in groups.

To the extent possible, techniques were selected which approximate face-to-face oral communication or which take into consideration actual problems of such communication. For example, responding to questions, carrying out commands, and directions, pointing to pictures of objects. Also the sentence or utterance was utilized as the primary unit for testing and not words in isolation.

Scoring each language response of the child requires only a single observation, a single judgment.

For the social and personal adjustment inventory, a list of behavioral characteristics was compiled which is regarded as culturally fair. For this, standard works on the psychology of adjustment were consulted. Several anthropological descriptions of Tex-Mex culture and a number of behavior inventories were consulted. The inventory is given to children systematically in an interview by the teacher in class.

So the battery consists of three instruments: An English proficiency test in two forms, a Spanish proficiency test in two forms and an inventory of personal, social and emotional school adjustment.

The proficiency tests are broken down into five main parts: Listening comprehension, pronunciation, vocabulary, grammar and reading. And these five parts are broken down into twelve subtests in which they give some three hundred and thirty-five items. I don't think it's necessary to go into the additional tests.

Each section of this test is introduced by practice exercises. The test begins only when the examiner is sure that the child demonstrates his ability to perform the tasks expected of him.

For the adjustment inventory, it consists of two parts: The first is an interview conducted by someone other than the child's own teacher. The interviewer is asked to make certain judgments about the child's behavior in the interview. The second part is a record of the child's behavior in school. His teacher marks the frequency or extent of selected behavioral characteristics.

The trial testing has been going on for some time. These tests will be used first in the Del Rio program for the end of program evaluation of the bilingual program which involves eight classes of students. Four classes are experimental and the others are not.

DR. BOGER: If no one has anything further to ask of Ed. I think we can move along to Charles Spellman's work. Chuck has been doing very interesting work with some of our Head Start centers.

MR. SPELLMAN: I have three transparencies that I would like to show.

STIMULUS PREFERENCE---COGNITIVE STYLE AND CONCEPTUAL
CONFLICT IN THE CULTURALLY DEPRIVED CHILD

Charles M. Spellman
The University of Texas

The purpose of this study is to explore cultural differences in cognitive styles among different ethnic groups. In particular, I am interested in what I am calling a "perceptual style" or "characteristic preference" for certain kinds of stimuli. This has been called by some people "observing responses." We are most interested in characteristics of the stimulus which the subject attends to, first, when he sees it.

Past studies have always shown that very small children attend to color first. When they get to about age six, they turn to form. This has often been taken to be a universal maturational process which everyone, eventually, went through.

All of these studies were done with white children, middle class or above. More recent studies have been done with American, German and French children and they seem to show the same thing. Preschool children have attended to color first. As they got into school, and went on to adulthood, they preferred form. So this was always seen as a correlate of cognitive growth.

Very recently, however, a study was published by Suchman using the instrument I use plus two other instruments with over a hundred African children in Nigeria. She found that out of over four hundred test scores, only about twenty-five showed form to be preferred. The others all preferred color. These subjects ranged all the way from the very young up to adolescents.

DR. ZIGLER: The problem with Suchman's stuff is the way she presented it. Mike Cole, now at Riverside, California, went out to Nigeria just a few months ago. I don't know if he went into the form and the color stuff, but you might want to contact him. One thing they found, though, was that the presentation made a lot of difference. They got good data with one type of presentation and very primitive stuff with another. I certainly suggest you see what he's got.

MR. SPELLMAN: I would be very interested in finding out.

DR. ZIGLER: He's at Riverside, California.

MR. SPELLMAN: I do know that Suchman used the same instrument in Africa as she used in the States and came up with quite different results with the two different cultural groups. In any event, I went to these various places: four places - Oklahoma, Utah, Arizona, and New Mexico - and I came up with four broad cultural categories. I guess you could lump the Latins and the children of Spanish descent in one group, the Negroes in one, the Indians in another, and two groups of Anglos, and I came up with very different results for these cultural groups.

The presentation, I am quite confident, was the same for all of these. The two Indian samples came from two different parts of the country; one from Arizona and the other from Anadarko, Oklahoma. The two Anglo samples that you see here on one line, came from Salt Lake City and Anadarko. The Latins came from New Mexico, the Spanish from Salt Lake City. And the Negroes from Anadarko.

MR. SUGARMAN: These are all disadvantaged children?

MR. SPELLMAN: These are all members of Head Start classes. They would differ from the subjects of previous studies, in that regard.

MR. SUGARMAN: Are you interpreting these results as cultural?

MR. SPELLMAN: Tentatively. I have no clear cut answers on this.

DR. ZIGLER: Well, you have to control for cognitive development, which is the alternative interpretation. The thing is, the age of the child is not a developmental measure of cognition, but the MA of the child may be. We know, for instance, from the IQ's that we saw earlier, that your Anglos are the highest IQ group. This means that they also have higher MA's. This is your best measure of cognitive development. The Anglos are the ones that are showing preference for form, and that should go along with higher cognitive development.

DR. COOKE: The MA's are about the same.

DR. ZIGLER: They are not.

DR. COOKE: Yes, they are. The Anglos were the youngest and had the highest IQ. I think if you do the arithmetic, you will find their MAs were the same.

DR. ZIGLER: Well, I would have to look at that. I am just remembering that the Anglo's had the highest IQ. What I am saying is that you have to equate the children on MA.

MR. SUGARMAN: Are these the same children that appear on the charts we previously looked at?

MISS PAINTER: Yes.

DR. COOKE: Then I am right. They are the same.

DR. OSBORN: The youngest would have the higher MA and the older the lower. They would average out.

DR. ZIGLER: If the MA's are the same, but, to interpret that culturally, you would have to demonstrate to me that the MAs are the same. Then the rest of the variance could indeed be cultural. Let me again point out to you that IQ is not a measure of cognitive development. A two year old child with an IQ of 70. So it's the MA that you have to plot.

MR. SPELLMAN: Well, there are no consistent correlations with IQ in any event.

DR. ZIGLER: There ought not to be.

MR. SPELLMAN: All right. The same holds true with the Caldwell Pre-School Inventory. There are no consistent correlations with that either.

One point which might be made, is that nearly all subjects, 83 per cent in fact, were "uni-dimensional" in their choices. So the preferences, you see, are averages for the groups. Only 17 per cent of the children showed an equal preference for form and color on their own individual tests.

DR. ZIGLER: Did you mention what these tests are?

MR. SPELLMAN: Yes. It's the test right here. There are 40 cards, you see. This is my "B team" set of cards. It's not as well done as the other ones. I would ask the child which two are most alike. If he pointed to a certain two, then I would mark him down as having a preference for color. There are four cards, none of them duplicated each other. The cards are symmetrical. Half of the cards have to do with a symmetrical form; half of them have to do with saturated color, half of them have to do with unsaturated color. I have found no kind of trends among the different categories at this point.

MR. CARLTON: I am not educated in this thing and may be very naive. But don't you have a built in bias, when you have to say, a circle and a square, in terms of asking a question regarding size? Aren't you almost inevitably going to have a lower response on that than if you have, say, a square and varying size rectangles?

MR. SPELLMAN: I have a bias against what?

MR. CARLTON: Against size? It would seem to me this would be the one that they would express the least preference for.

MR. SPELLMAN: That's correct.

DR. OSBORN: But, this adjusts itself because you have to go to all of them. All of the forms are not the same.

MR. CARLTON: Oh, they are not?

DR. OSBORN: Show him three or four cards.

DR. ZIGLER: What do you say after the kid makes his choice?

MR. SPELLMAN: I don't say anything. This whole battery, I found, can be given in five minutes.

DR. ZIGLER: He gets no kind of indication that he is right or wrong?

MR. SPELLMAN: That's right.

DR. ZIGLER: You don't say anything punishing?

MR. SPELLMAN: Not a thing.

DR. OSBORN: Not even a "uh huh" or "huh"? I am thinking of Greenpoon's effect. The "huh" is important.

MR. SPELLMAN: I have been very careful not to make any kind of response at all.

DR. ZIGLER: Do you do any kind of switching to see just how the reliability on this is? Do you do any kind of construct of validity to show that there is such preference?

MR. SPELLMAN: I haven't yet, but we will do it.

DR. ZIGLER: Could I suggest one to you? There is a test that's good for kids this age, and I have used it myself, you get a set of cards that could be sorted either according to color or form. There are five colors in five shapes. You would have a red, green, orange, yellow, blue triangle; green, red, yellow, blue, orange star; green, yellow red, blue, orange diamond. In other words, you have twenty-five cards. You hand it to the kid and give him five boxes. You say to the child: "Okay. I would like for you to sort these." He can sort them for color or for form. With kids in the five to seven year range, the first sort presents no problem at all. It would be very interesting to see if they actually go about another problem the same kind of way.

The thing you can then do with that test, is to mix up the cards again and say to the child: "Now, I would like you to put them together another way," and see just how! Then you have a trial factor which is a bit more sensitive to find out just how much of a bias it is. If a kid immediately switches over on you, it looks like it's some real built in kind of thing we have to worry about. If he switches on the very first trial it looks like either way is right. All the kid is doing here is to pick on something. It would give us some indication of the strength of this style that you are studying.

DR. OSBORN: I thought that was what Segal did. He found the kids did have difficulty in the second sort.

MR. HOOD: This is the kind of thing that appears in the New York City project. ETS has looked at this type of experimental test.

DR. ZIGLER: What I've suggested would give you a little validity evidence.

MR. SPELLMAN: Children who are uni-dimensional, say in terms of form, had no trouble at all handling these tasks in terms of form. But if they were asked to do them in terms of color, they made a lot of mistakes. The children could not learn as well with these stimuli, those that they did not prefer.

DR. ZIGLER: I'd be careful of that one - that they don't perform as well. There is a fascinating study on this concept of switching colors and forms. What that study shows is consistent with the work of Terrell - that lower class kids don't make the switch, not because they can't, but because they are not very motivated to be right for right's sake. If you give them a tangible reinforcement to make this switch, they look just like middle class children. You also have to take into consideration, in this

kind of research, the fact that you have different reinforcement hierarchies operating in your two populations. The arbitrary reinforcement that you are dispensing is probably effective for one group and ineffective for the other. So you have to be careful for that each group gets the optimum reinforcement. It gets tricky.

MR. SPELLMAN: As I recall, earlier studies show that the more intelligent the children are, when they prefer form, they have a great deal of difficulty in accomplishing tasks which call for color discrimination.

DR. ZIGLER: Yes, don't misread me. I am sure you are on an important variable, and I agree with you. You have to be sure, though, that you are controlling for other variables that influence this kind of performance, so that your conclusions can be as good as possible.

DR. GOTTS: I have the mental ages for you now. The mental age for the Salt River group is 55.84 months.

DR. ZIGLER: Which group is that?

DR. GOTTS: This is Indian. The Anglo sample, just taking the Salt Lake City sample, has an average mental age of 58.14 months. If you just took the Negroes in the Austin sample, the mental age of this group would be 62.52 months. And if you took just the Latins at Carlsbad, the mental age would be 54.31 months. You still get a very large effect, in the area that he is interpreting, that is independent of mental age. He is getting things other than in the expected direction. These are just means. I don't know what it would look like within the groups, of course.

DR. COOKE: Do you control for color blindness?

MR. SPELLMAN: No, we haven't. I don't suppose there is any way to do this. Because these children just coming in, I don't suppose this has been determined.

DR. OSBORN: What about the children's Ishihara? Wouldn't that work?

DR. ZIGLER: Ask them to point to the colors on an independent-- whatever colors you have. That's all you have to do.

MR. SPELLMAN: Has everyone looked at the cards to his satisfaction? At this point, I am without as complete an analysis

as I intend to do as soon as we get the data punched up on the computer cards. I am thinking about cultural differences in perceptual style. The implications are that some children, specifically those of groups which prefer color, come into the school room not prepared to do the tasks, or, at least, not as well prepared as the Anglo's are.

DR. ZIGLER: That's another empirical question. Do you have the correlates?

MR. SPELLMAN: As I said, my statistical analysis is very rudimentary.

DR. ZIGLER: You see, it makes a lot of sense if there is a developmental phenomena and there is some gene factor and you say well, these kids are developmentally lower and reading is a developmental task and you have some genes factor and expect a correlation. To the extent that something like this is cultural, though, you are arguing both sides of the street simultaneously. There is something in the culture that is non-developmental, non-cognitive, that is producing some kind of preference that doesn't affect their reading skills at all. I would raise with you the empirical matter of what are the cognitive correlates of a cultural preference?

DR. PIERCE-JONES: I think this is a question that has to come up for examination.

DR. ZIGLER: Yes, it's quite an empirical question.

MR. SPELLMAN: That's correct. I have used the word "implication."

DR. PIERCE-JONES: Right. I think he meant hypothesis.

MR. SPELLMAN: I meant that, believe me. I have no concrete answers to these things as yet.

DR. ZIGLER: I think this is intriguing and worthy of being chased down, and it could very well be a tremendous dimension because there are a lot of factors like style, very simple kinds of things of this sort, that could be operative and experiential or cultural in their composition, in there--I am sorry--genesis. And I find no problem with this backwards, forwards and sideways.

DR. COOKE: The most passive group are the ones for whom it doesn't seem to make much difference. So what? Whether it is a kidney or a bright red bean, it's all the same.

MR. SPELLMAN: You are talking about the Indians?

DR. COOKE: Yes.

MR. SPELLMAN: But remember, eighty-three per cent of the Indians were uni-dimensional. It just so happened as many preferred form as did color. And it just averages out.

DR. COOKE: I see.

DR. OSBORN: I think that is very interesting. I can't imagine why, why you would get this.

DR. ZIGLER: A cognitive hypothesis, a cognitive period, would handle that very nicely. There is a changeover. Half of them are there and half of them aren't. But there is also a possibility of a cultural phenomenon here, where you have a group that really doesn't give a damn; there is nothing in their culture that makes for a preference. Maybe there isn't anything that makes for interest. So all that you might be measuring is disinterest combined with consistency.

DR. OSBORN: I wonder if what you pick up is lack of commitment.

DR. CARLTON: If your hypothesis is validated, what does this mean in terms of changes in the classroom? Would it mean printing in four colors or what?

MR. SPELLMAN: This was my first thought.

MISS CAUTHEN: Timing your introduction of things. And more experience in what the children would consider more functional operation of certain kinds of play and activity that would help to speed up. So that they would care more. We really would get more into preferences that have meaning to them in the child culture. We keep ignoring the child's culture which is a factor, you know, as well as these ethnic group differences.

DR. ZIGLER: It's clear that this kind of thing is important in learning. Mort Weir, from Illinois, has just published a study that's been replicated by others which indicated some big effect that they thought was verbal mediation. These things do tend to be terribly important in the child. How they are developed. How they grow up. Obviously, there is a cognitive component in all of this. What is this cultural component? There's some interesting stuff of this sort.

What you need, I think, is probably an anthropologist. There is a fascinating work by a man from Iowa, I can't remember his name, who found a certain society where they don't use the rectangles. If you use rectangles in that society, you just "throw them for a loop." He is an Africanologist.

At any rate, it would be interesting to find out what, in these cultures, makes for this sort of thing, if it is, indeed, cultural. I'd like to see a little bit more of this-- rather than just the straight developmental work, rather than the IQ test, itself. Just how good a measure is the MA for general level cognitive development?

DR. COOKE: I would think this would be the sort of thing you would want to take to the families of these children, because you know, you are assuming that this is a developmental thing - because some people took some middle class kids and that the child is older in one way you look at it. That may not obtain in these particular families. We might learn a good deal about what is the big stimulus to this particular family and that is why it's a big stimulus to this particular child.

DR. ZIGLER: I am sure there are such cultural differences. We have found something like this in the kids matched on MA, but I don't remember which way it went. We used institutionalized retarded kids who had equal MAs with normals, but as to color and form, it's not just developmental. My hunch is something about institutional living has something or other to do with this.

MISS CAUTHEN: I'll bet it would make a difference, too, in classrooms, depending on the materials and teaching style. I'll bet you could get a switch in a very few months with the kinds of experiences the children would have in very practical places.

DR. OSBORN: Well, I think this is where some of Segal's stuff comes through. He raises some real questions on this. The intensity of the color has a lot to do with it, too, and the intensity of the difference between forms.

MR. SPELLMAN: Let me make two comments: One is that these cards try to control for a child taking a set and sticking with it. On eight of the cards, say, if the child starts out choosing color, well, on eight of the cards there is no way to match color. On eight of the cards, there is no way to match for size.

DR. OSBORN: Are these presented in a certain order like, say, from one to fifty?

MR. SPELLMAN: No. I have mixed them up. I have tried several orders and it doesn't make any difference. Suchman found no difference. And she found a test, re-test reliability of .97.

The other comment that I might make in passing is that in Salt Lake City, I took two samples, children of Spanish descent and Anglo's. They were in the same classrooms, all mixed up together, and they lived in the same city. Yet they were diametrically opposed in their preferences. Both of these groups preferred size significantly more than any of the other groups, and two groups preferred size.

DR. ZIGLER: Well, let me go way out to left field just to cover all the bases. Cultural differences could very easily be genetic differences in many cases. And there is beginning to be a slough of work now being done that leads me, at least, to entertain the possibility of some different wirings in different ethnic groups. Of course, this is all very tenuous.

DR. COOKE: The best way to get at that would be through family studies.

DR. ZIGLER: I am just saying that there is some work now being done that is quite surprising - by Birch and his group - on some of the ethnic groups in New York City. It really looks like he is beginning to uncover some genetic differences in style, in tempo, and these kinds of things. It wouldn't be totally nuts to think in terms of some kind of stimulus preference related to "wiring differences" in the organism, though I, at this point, would take it as very tenuous.

DR. PIERCE-JONES: My feeling is that Charley is opening Pandora's box for himself as his work progresses. (Laughter). Dr. Caldwell has to leave to get back to the Gulf Coast. He says he hasn't much to say, but what he has to say, I'd like him to say.

POSSIBILITIES FOR PREDICTING SCHOOL
LEARNING DISORDERS FROM EARLY LIFE HISTORY

Bill S. Caldwell

Child Development Clinic

The University of Texas Medical Branch

I am with the Medical Branch in what we call our "Child Development Clinic." And so I just wanted to point out some of the areas where we hope to expand some of the studies that are going on at the main campus.

We are particularly interested in our clinic in evaluations of the pre-school child. You mentioned genetic studies, Dr. Zigler. We have a division within our clinic which is now making a study of some forty children from a genetic standpoint. We also have available a school for the cerebral palsied. And we have other opportunities to study children comparable to any medical school.

One of the reasons I went to my present professional setting, frankly, is illustrated by the studies reported here today. While we have mentioned this matter from time to time, there has been very little consideration given to the possibility that much of the differentiation of disadvantaged children from others may not be due to cultural differences at all. In other words, I don't hear much consideration being given to plain physical differences, to, differences that can be picked up, perhaps, before a child is born.

We are interested in developing questionnaires--this has been done in other places, I realize - to see if we cannot, to some degree, predict when a child will have difficulty in school. The questionnaire would include a pre-natal, post-natal and a birth history. We think this could be an excellent way of screening. In the situation where I work, we see something like four hundred children a year for each of whom we do a complete evaluation. A fairly sizeable percentage of these are clinic children, which means disadvantaged children, so that we are interested in devising better techniques, better instruments for evaluation.

I have devoted some time this year to the Observer Rating Form, and Bob Boger and I are working on still another study that I think he may tell you about later. But, over half of the children we see are children who come to us with various types of learning difficulties. And this is what interests me.

MR. CARLTON: Could I ask a question on your belief that predictions can be made in terms of able to cope and not able to cope or able to cope very well or able to cope excellently?

DR. CALDWELL: I think we should be able to predict whether or not a child may have such difficulty in school that he will be referred for services by a classroom teacher. Using this criterion we believe we can predict it. We are doing what many other people are doing. We believe that we can do this on the basis, first of the questionnaire that I mentioned. We know, on the basis of an individual's history.

DR. PIERCE-JONES: Given to parents?

DR. CALDWELL: Parents and other informants. Second, we use neurological information and then specified psychological tests. So we have been in this business a long time and there is no question that you can do this. You can say that those children with such and such things are going to have trouble.

The question which nobody has satisfied me on, is how many of those children do you or don't you identify, because you never see them? That's the real problem.

In the Galveston school district, which is a rather small school district, they have something like twelve hundred children entering school each year. We are working out a pre-school screening program in which nurses give simple neurological tests. This kind of thing is done elsewhere, of course, but we have a population which we think we can follow for a period of time. I am sure we will see attrition, of course; we lose a lot of children. Out of twelve hundred last year, only nine hundred were included in the pre-school screening.

DR. COOKE: There really has to be, however, a fair size group of children who aren't identified - I mean they aren't identified using our present techniques. Somewhere along the line, somebody is throwing in some customers who aren't detected by the techniques that we have - by neurological examination and very exhaustive pre-natal records, et cetera. Somehow, there are some that are not coming in. And, of course, you don't have to have, when you get right down to it, you don't have to have a very big negative error percentage to account for this. Five percent is pretty good for a biologic test of any sort. And all you have to have is a five per cent miss to account for, you know, the kids who are retarded in a sense when they are in school, but weren't picked up. So it's not too surprising that you don't pick them all up.

I think it's good if you've got a group you can follow along, because I think it may be one of the better ways of confirming the increase in defect with age, but it doesn't convince me at all, one way or the other, that the defect wasn't there earlier. That's one of our present difficulties.

DR. CALDWELL: When you say the defect was not there. Earlier than what point?

DR. COOKE: Earlier than six or seven or eight. That the defect was there at birth, if you want to put it that way, pre-natally or pre-conception.

DR. CALDWELL: Right.

DR. PIERCE-JONES: Let me interrupt again and switch your ears if I may. One study we have involves "diffusion into families". This is David Mandel's work. Dave has been buying baskets of fruit and many other kinds of funny things, writing notes, interviewing parents, and a number of other things.

Dave, why don't you tell about it.

MR. MANDEL: Okay.

DR. OSBORN: Where are you located in the book?

MR. MANDEL: Oh, in the first book, I am located on page sixty. In the second book, I am located on page seventeen.

DR. OSBORN: Could I ask one other question? What is your discipline that you come from?

MR. MANDEL: I have a BA in Psychology and spent a year and a half in the Psychology Department and now I am getting my Doctorate in Educational Psychology.

INFLUENCING PARENT AND TEACHER ATTITUDES
THROUGH REWARDING CHILDREN

David M. Mandel

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The question to which I addressed myself earlier in the academic year was, "What changes occur in teachers and parents as a function of changes occurring in their children?" It seemed to me, taking theoretical notions into consideration - especially those of Sears and Erickson in which they stress the importance of the mother-child relationship - and looking at the developmental research, most of the studies seemed to have been uni-directional. They were concerned primarily with changes in children, and with correlating those with parent's personalities, et cetera.

If we are to effect changes in children's readiness for formal schools through our compensatory educational programs, as some writers have suggested, it really is necessary, also, to effect changes in the parents and the teachers in the same directions. This seems obvious if we are to maintain any of these changes, longitudinally speaking.

One of the other problems that I was wrestling with was the function of positive reward. In this regard, Dr. Zigler has mentioned the differential effect of motivators in the lower class or the disadvantaged group. Other writers have also suggested, or actually utilized, these notions in their studies. Terrell did some work, Terrell and Beasley, finding, essentially, what Dr. Zigler has found, namely that concrete material rewards could produce effects in socially disadvantaged children where abstract, non-immediate rewards, have little or no effect on behavior.

One of the ways in which I tried to tackle this problem, in taking off from the theories of Sears and Erickson, was to try to find some index of early deprivation in this (Head Start) population. If the quality of the mother and child relationship was maximal, if there was a great deal of interaction, then the mother should know those things which the child can do, assuming that she has paid attention to the child. On an experiential level, she should know what he can do, what he can accomplish in most tasks that he is given. On an emotional level, as Erickson's theory implies, and as others who have investigated the effects of early deprivation have demonstrated,

one index of this may be gotten from the attitude that the parent, or mother, holds toward the child.

I have used two devices in trying to get these indices. They are the Pre-school Inventory and the Behavior Inventory, the latter being utilized to get measures of attitudes toward the children, and the former for "experiential measures" of the child. What I did was take sixty-four out of the eighty-five items in our form of the (Preschool) Inventory and split them into two forms. I used two forms of equal length each having thirty-four items. One form was used as a pre-test, the other as a post-test. All these forms that I am going to mention, I had translated into Spanish. Mr. Espinoza, and several others who are Mexican-American born, helped in translating these, giving examples, translating examples, and so on.

I had approximately nine interviewers, six of whom were bilingual. They spoke Spanish and English fluently, and they interviewed a sample of seventy-seven kids. My ethnic breakdown was fifty-three Mexican-Americans, twenty-four Negroes and thirteen Anglo's. I wasn't interested in the Anglo's because that sample was so small.

DR. PIERCE-JONES: These interviews with parents were all in the Head Start classes?

MR. MANDEL: All in the six sample classrooms of the Head Start program here in Austin. The six classrooms were from three different Centers. There was one teacher per two classrooms, morning and afternoon session.

Prior to registration, or prior to the child's starting in the program, the parent was interviewed- some during registration. Others who were contacted by mail were asked to come into the school for the interview. The interview schedules were the thirty-four items of the Pre-School Inventory and twelve items from the Behavior Inventory.

The criterion for selection of the twenty-four items finally chosen from the Behavior Inventory was 100 per cent agreement among four judges--two of whom are extensively experienced in clinical work - and myself and Mr. Espinoza.

The items of the Behavior Inventory were grouped into categories which were decided on a priori bases. The categories were very similar to Dr. Zigler's categories: Sociability, independence - dependence, emotionality, persistence or achievement. The criterion for selection was 100 per cent agreement between the four of us, and this process turned up twenty-four items. Those were, in turn, split into two twelve items interview schedules. The parent

was asked the Pre-School Inventory items first. She was simply asked, for example, and in either language she wanted to use, "If I asked your child what his first name was, what would he say? Would he be able to tell me?" "If I asked him--" and cards were put out with the little boxes and as another example, "If I asked him to place the black card on the red box, could he follow that direction?" The interviewer scored the responses as though he were actually administering the test to the parent.

Then the parent was asked twelve items from the Behavior Inventory. By the way, these twelve items were paired for positive and negative response directionality on these dimensions. As yet we haven't analyzed the reliability data on this form or for other forms in existence: The parents were asked, again in either language, with the examples given, the questions appearing on the Inventory. We made some modifications of the Inventory because of some of the difficulties that had been noticed in old forms as filled out by teachers. Dr. Linn mentioned some of these troubles earlier.

We couldn't train the parents to score this Inventory for the child, so we changed it somewhat. What we did was construct a response scale for each item that we presented. The Parent Behavior Inventory form presented the items in both Spanish and English with examples below each, again in both Spanish and English, and below that appeared the response scale. Each item response scale differs from each of the others in that the marking categories were rotated. The first scale: "very much like," "somewhat like", "very little like", "not at all like", appeared in the usual form. The next scale, however, starts with "somewhat like", "very little like", "not at all like", "very much like" and "somewhat like." And then it's rotated again where the third one is "very little like", "not at all like", "very much like" and "somewhat like." The scales were rotated in order to reduce the probability of a response set even by the examiner, when he scored these.

Sometimes when a parent was asked: "Is your child usually carefree or happy; does he rarely become frightened or apprehensive?", the parent said, "Well, I don't know. Maybe." If the interviewer felt that this was more of a "negative" response than a "positive" one, he would choose the two on the lower end of the scale.

DR. ZIGLER: Didn't you have trouble with some of the wording with some of these parents? A word like apprehensive, for example?

MR. MANDEL: We didn't use words like that. We had to modify a lot of the wording. In fact, some of the items were difficult

even for the teachers. "Is he usually bashful?" "Does he usually respond to frustration because he is bashful, withdrawn or afraid?" Is he suspicious?" These confused them and necessitated some further explanation. The wording was "watered down" in both Spanish and English for this study, both for the examples and in the actual wording of the items.

Next, the parent was told that her child might bring some things home from school.

DR. PIERCE-JONES: This was after the initial interview?

MR. MANDEL: During the initial interview, after we collected the Pre-School data and the Behavior Inventory data. The parents were told that their child might bring things home from school as part of our program.

DR. ZIGLER: Did you ask the parents if it was all right?

MR. MANDEL: No, because we figured that children often bring things home from school anyway.

DR. ZIGLER: Talking about ethics, this is the type of thing that worries me. You never give middle class kids candy, because their parents don't want you to. Why is it all right, then, to give these kids anything to take home? I would be very concerned that some parents would see this as insulting, that "Do you think my kids don't have enough to eat?" I think that a very important aspect of a study of this kind would be to say, "We are going to have an experimental type of program," and then explain to the parent why the child is bringing things home and get the parent's permission.

MR. MANDEL: We didn't mention what the kids would be bringing home, although I really see your point. We did mention that it would be part of this particular Head Start program that they might be bringing things home that they had earned in school. This was the way it was approached.

DR. ZIGLER: That is not so bad. I'd like to see parents approve, though.

DR. PIERCE-JONES: Yes.

DR. ZIGLER: Because middle class parents -- you hand out some M and M's sometime, the parents will go right through the roof. They don't want their kids to have this. You just assume that the

lower class parents do and I don't think that is very fair. I don't think you would get much static out of them. I think you are on safer ground.

MR. MANDEL: I didn't get any static, and I did check on my manipulation during and after my post-interviews.

DR. COOKE: Why did you use food?

MR. MANDEL: Well, I wrestled for weeks with this problem, among many, of what is a relevant reward. And here's where I was wrestling with the ethics part, with questions that Dr. Zigler is raising. I thought about, you know, sugar, flour, things of this nature that the family could use. I thought about green stamps.

DR. COOKE: What about money? Just plain money?

MR. MANDEL: Money, too, except that I asked, I have several colleagues and friends here who are Mexican-Americans from all over Texas, attending the University, and I asked them, "What would be relevant here?" They told me that money would be more of a "slap in the face," and not only to the parents. They argued that the kids wouldn't see the value of it over anything else unless it were a ten dollar bill. Then it would raise all sorts of suspicions as to what is going on in our schools. They pointed out, very quickly, that money, unless it were a very legitimately earned and especially a gross sum, would be very bad.

DR. COOKE: Is that a fact?

MR. MANDEL: Well, I don't know whether it's a fact, but you know, fifteen cents or a quarter would not be at all appropriate.

DR. COOKE: At this age?

MR. MANDEL: At this age. Especially at this age!

DR. ZIGLER: Food is much more acceptable as a gift anyway.

MR. MANDEL: That was part of the rationale behind it.

DR. COOKE: I am sure this differs enormously from one cultural group to another. For children living around the Hopkins Hospital, five or ten cents would probably be the most meaningful thing that we could give.

MR. MANDEL: I am not interested in whether the reward is relevant to the kids at all, because I am not interested really in - although I will gain it - measured differences in early functional reward in the children. What I am really interested in is indirectly rewarding the parents through the child to change their attitudes both towards school and toward the child himself.

DR. ZIGLER: There is growing literature on this business of the reinforcing hierarchy. One of the things you do to make your test as powerful as possible is not to think in terms of one particular reinforcer because it has some general okay. You understand Witreal's work on simply doing comparisons with five or six different reinforcers to find out what is useful - pit one thing against another thing and get for every family what is for that family the optimal reinforcer. This gives you a heck of a lot more powerful hold on your variable than simply selecting one that you think is going to have some relevance. If you don't get results then you can always say you didn't pick the right one.

MR. MANDEL: I had initially attempted to go out and ask people in the community what would be worthwhile to them - whether they would prefer receiving something like stamps or movie tickets to a Spanish show. This was another possibility we had in mind until they closed the one Spanish theatre in town. Family movie tickets would probably have been the best, or so I was told by several Mexican-Americans in the community.

DR. NIEMEYER: Well, didn't you want the gift to appear as if it were a reward for the child?

MR. MANDEL: I wanted it to appear as something that the child earned in school.

DR. NIEMEYER: You wouldn't give movie tickets to a four year old child?

MR. MANDEL: No. Movie tickets for the entire family. Over a period of time, get children's admissions and adult admissions so the whole family could go. It would be something that showed that the kid was "a breadwinner."

DR. ZIGLER: Another possibility would be--I have seen this in other areas around the country---to hand out some kind of token so the parent and child could show up at the Head Start Center and pick from a whole slough of things.

MR. MANDEL: Right.

DR. ZIGLER: It would really give you another handle on the things. You know, somewhere within this range of awards there is something valuable. And also, it ties the kids and parents together in the selection process if the kid gets to pick it out and the parent helps pick it out. It could be money. It could be--there is no insult there, because they actually trade their token for whatever it is that they want.

DR. OSBORN: Incidentally, we did this. We had a room we called the "Paradise Room." I don't know what the Spanish equivalent would be as to that. But it worked out very well. And it did bring the parent and the kid together. And these ranged all the way from toys up to household articles.

DR. ZIGLER: With fruit as a reward, what happened?

DR. PIERCE-JONES: The office gets crowded with baskets of fruit, I'll tell you that! (Laughter)

MR. MANDEL: In conjunction with what you are saying, Dr. Zigler, in one of the post-interviews a side comment that one parent, Mrs. Fernandez, made to one of the interviewers was that she didn't like avocados. (Laughter). You know, we make the assumption that Mexican-Americans like avocados, but we are all wet in regard to some individual families. I think that is a very legitimate thing to think about.

At any rate, along with the bag of fruit which was securely stapled with a note affixed to it stating that "Johnny has earned this for doing good work in school today." He was also given, by the teacher, a little plastic toy with the bag, a ten cent toy, and these toys were kind of cute and meaningful for the kids at this age. There was a toy for each child - both female and male toys were given to the teacher to dole out to the respective sexes.

The teachers were told that they could set the toys up as for a competition with the kids if they wanted to. That is, the children who did the best could have first choice of the toys, et cetera. The three teachers that I was working with did take this approach. Some of them displayed the toys and that sort of thing.

Each child received three pieces of fruit, various types of fruit per bag, twice a week. The days varied so that they didn't know on which day they were getting the fruit until they came into a

classroom and saw it. They learned very quickly where it was, however. In one morning class, as I came in, everyone thought I was the fruit man doing something for the teacher. They called me "Santa Claus." They knew they were getting fruit that day. Because, regardless of what they did, each child got a bag of fruit in one of the classes.

I was very concerned as to whether the classroom children ever saw one another as one classroom left and the afternoon class came. I was told that they did not see each other. So the kids, presumably, did not know that the other class was getting fruit. That was no problem, because the afternoon class gathered fruit at the end of the day. One of the days, the morning class got fruit, and there was no overlap. I did know, and suspect, and wanted the parents to find out that some kids in the school were getting fruit and others were not. And this did happen. At one parent meeting that I didn't attend - the parent meeting at one of the schools - there was some commotion, because several of the parents had these little notes with them, flashing them around to the other parents, saying, "Look what my kid did," and "Look at what my kid is earning." One parent was kind of irate, because her son "wasn't doing well in school" and her neighbor's son was doing very well. And when the chairman of the parent's committee said, "Well, how do you know your son isn't doing well?" She said, "Well, he is not earning any fruit." "Mrs. Smith's child is." Our intervention went on for five weeks, twice a week. Let me backtrack. The teachers also filled out the Behavior Inventory for each child, the complete fifty items inventory, during the second week of school. It took one and a half weeks to familiarize themselves with the children, so they would have some basis for rating them. At the end of the five weeks, the parents were again contacted. They had been told in the initial interview, of course, that we would talk to them again later about the child's progress and ask them some more questions about their children.

The parents were contacted by mail, and were asked to come to the school for another interview. As expected, it was a little difficult getting some of these parents in the second time. We have, now, three more left to get and that's it. We have interviewed approximately seventy parents. We lost seven in our sample.

Right after the parents have been interviewed, the children are being tested again. During the post-interview, I have another schedule which I ask the parents. Form B of the Pre-School Inventory, what I call the "Form B" post-test, is administered and then the Behavior Inventory Form B is administered. Then I administer Form A of the Pre-School over again, instructing the parent that their responses are the opinions they have concerning children in general,

e.g., "How do you think children in general can do these tasks?" So that I get a difference score, a discrepancy score.

DR. ZIGLER: What did you say you were measuring them for?

MR. MANDEL: With this measure, I can get a measure of, an assessment of how positively or negatively they rate their child with regard to other children. I get an attitude of positive or negativeness towards their own child as compared with "children in general" at that same age.

DR. PIERCE-JONES: Children in general is a kind of reference point.

DR. ZIGLER: So if they are positive compared with children in general, do you think you have a good effect? Negative, you think you have a bad effect?

MR. MANDEL: No, I did not, I did not get a pre-measure on this so I really can't say that this is a function of--

DR. ZIGLER: What are the definitive measures?

MR. MANDEL: Oh. Oh, I see. Change in discrepancy score is my definitive measure.

DR. PIERCE-JONES: Between where? The discrepancy?

MR. MANDEL: Between the rewarded high, high initial discrepancy and rewarded low discrepancy kids.

DR. ZIGLER: Well, do you think with this high discrepancy, that following food reward the discrepancy will close up?

MR. MANDEL: Yes. Now, the way I broke my groups down initially was this: In the parent-child dyad, each kid received two scores. First, he received the score his parent--his mother--gave him and, second, the score he actually earned. So we have a "real" and an "attributed" score for each child.

DR. ZIGLER: You lack a very important control on this study, in my estimation, because you don't know, from this kind of design - you've only got these four groups, right?

MR. MANDEL: Right.

DR. ZIGLER: Then you don't have any kind of base against which to assess the change. For instance, one group of parents knows that other groups of kids are getting rewards and their kids are not, so that "no food reward" is not any simple control. It's a group that may be becoming angrier and angrier at their child for his stupidity at the school for not recognizing his worth, and so on.

It could be, you know, that if you had a control group someplace else where there wasn't any contamination, you would discover that control group was doing exactly the same thing the food reward group was doing, and that you effect was not due to any positive influence of food reward. It might actually be due to the negative effect of your experiment. So that control is absolutely mandatory for a base line against which to set any differences between your food and no-food groups in their interaction as well as being variables.

MR. MANDEL: I can see that. Yes.

MR. SUGARMAN: Also, doesn't your "real" measure, as distinct from your "attributed" one, change also during the course of the program?

MR. MANDEL: Yes. I expect that to change. Now the direction of the change--

MR. SUGARMAN: Yes. As one goes up on a curve, do you expect the other one to go up, or do you expect the discrepancies to narrow?

MR. MANDEL: The direction of change is something which I haven't analyzed yet.

DR. ZIGLER: You'd like two more groups. You'd like a high and low discrepancy group that is actually a control group--

MR. MANDEL: Yes.

DR. ZIGLER: And that would give you that information, wouldn't it?

MR. SUGARMAN: Do parents over-rate or under-rate their children?

MR. MANDEL: I don't have that material with me, and I haven't really analyzed it yet, but this is one of the analyses I will be doing. I do have the change scores between the pre- and post-measures

for the parents. The changes in the discrepancies--not in the discrepancies but the change scores for the pre- and post-measure. If they do not differ between the two groups, the reward and the no reward group, that is, differ regardless of their direction, I may be able to use that as a control in my design.

DR. NIEMEYER: Could I ask you? You said, you just threw in a tiny piece that I can't fit into the puzzle when you said you suggest to the teacher that they might use the fruit as a basis of competition in the group. Didn't all members of the group get the--

MR. MANDEL: That was the toy.

DR. NIEMEYER: The toy?

MR. MANDEL: They all got a toy. But the competition was to be based on who got first choice of the toys.

DR. NIEMEYER: Oh, I understand.

MR. MANDEL: There were various toys. There were the same toys at different times over the weeks. They all had the possibility of getting each and every toy.

DR. ZIGLER: I think this is an intriguing kind of work. I really think there has been a dearth of this kind of investigation. What happens with parents with changed attitudes? They go home to these homes and this is something that has been underscored by Head Start in our research. I am intrigued by this research, and I hope you press hard on it. But you have to be careful with kids about the appropriateness of the reward.

For instance, I would, if I were going to go to this much trouble, instead of the competition, I would have the teacher rig things a little bit so that whenever a kid got a reward, he also felt he accomplished something. Because it's one thing when you went to a competition, you were the last one to choose and you take this home, you don't come home the same way as you come home with it saying, "Boy, I really was great."

So what I would do in a study of this kind is have a teacher--it's a very simple kind of thing - give them. These things go together; just make it a special point of that day when he gets rewarded that he gets a lot of verbal encouragement about how well he is doing that particular day and then Bing! - the reward appears. It looks realistic to the kid and it looks realistic to the parent and it saves you a little contamination.

MR. MANDEL: I was, you know, interested in finding out what happened to this fruit - as to whether it would get home or not. So I asked, at the end of the interview, "Did your child, so and so, bring home anything from school?" The interviewer was instructed to listen for the mention of fruit. If no mention of fruit was made, then the interview was terminated. But, if the mention of fruit was made - as well as paintings and drawings and valentines and things like this--the interviewer asked, "Oh, he brought fruit home. How often did he bring this home?" And the response was recorded. And then, "When he brought the fruit home, who usually ate the fruit?" And I have several categories: mother, father, child only, siblings, all children, siblings only, others, et cetera. I have been looking at this, but I haven't analyzed it formally. It seems, however, that all parents who were rewarded (with two exceptions) were aware that the child was bringing home fruit.

DR. NIEMEYER: What happened to the program in the school when the teacher stopped giving rewards?

MR. MANDEL: As far as I can tell, now, nothing, at least from the teachers' reports. During the intervention, I checked with the principal, as well as with the teachers, every time I went to the school - at least twice a week - as to any repercussions from the community, from parents, from the kids, and got nothing. Except right at the end, when the parent meeting, previously mentioned, was being called. It was right at the end of the year.

DR. NIEMEYER: How old are these kids?

MR. MANDEL: Five and six.

DR. NIEMEYER: With five and six year old children being given rewards, with some excitement surrounding the choice of part of the rewards, and when the rewards were not related to any real activity that could be perceived by the child, and then suddenly stopped at the end of five weeks without having a very severe effect. I just can't believe there couldn't be ill effects. In other words, I'd like to make a plea against this kind of research.

I say this hesitantly because it seems to be a personal criticism and I don't mean it as such. But this borders upon introducing poisons into children's systems. It also introduces poison into other kid's systems. Unless you did this--unless your control groups were off someplace where there could be no possible contamination. It's so dishonest, it seems to me, to give kids this kind of meaningless and unrelated reward. I am not terribly keen about

extrinsic reward when it is linked to reality, but I am not opposed to it. And I would be perfectly willing to play with it--to work with it. But I find this rather shocking. This type of research.

DR. GOTTS: I would like to raise a question here: Is this any less humane than giving a grade, which has no meaning, to the disadvantaged child?

DR. NIEMEYER: Well, at least the grade is distinctly related to the fact that you spell the five words correctly or incorrectly. You know, I think they are nonsense for any really good teacher. They are crutches for poor teachers.

DR. ZIGLER: I think I'd go along on the honesty business as I said a little while ago. You always have to justify research as having great potential good when you start being dishonest. This is the real problem that we are now looking at closely - looking at especially closely with respect to children. There is nothing, though, in your design that couldn't make these rewards honestly, and it would be a better design anyway. This other point, Jack, it's a problem. There are at least a hundred and fifty projects having nothing to do with Head Start around the country with which optimum procedures are being used to shape up children. I happen not to be enamoured with them. But I can't see that there is very much evidence that they have hurt anybody.

DR. NIEMEYER: I am not opposed to the reward system.

MR. SUGARMAN: I hate to be on the other side from you, but it seems to me if you had a situation in which you have, let me use the words that have value: "good parents" and "bad parents" - "Good parents" meaning those who really know what their children are like and are really supporting them and so forth - their discrepancy score is not great. And then you have--

DR. COOKE: That isn't a good definition for good and bad parents.

MR. SUGARMAN: I know it isn't. I know that. I've got to use something. Group I parents, have very little discrepancy and Group II parents have great discrepancy. And I presume that the objective here is to find a system which will convert Group II parents into something closer to Group I parents in terms of their knowing about their children and being supportive.

I would hypothesize that the Group II child tends to be a lower

performer than the Group I child. If that is so, then you need to do something to trigger the Group II child's beginning to believe and the Group II child's parent beginning to believe that something can happen.

DR. NIEMEYER: But Jule, on that we are not on opposite sides of the fence. I am simply saying, and I won't be indelicate in this company, but I can name a whole list of things, and at a certain point you would have a cutoff beyond which you wouldn't go in giving what would seem like a reward. You get into a matter of value judgment. I am just thinking that there are, I would think, hundreds of ways in which you could introduce rewards into the system and they could be related to something. I hate building unrelatedness into the lives of children who are already too unrelated.

MR. CARLTON: If you have three cars to give to ten kids or ten cars to ten kids. Well, if you have three cars to give to ten kids and you go on the basis of how they perform, then you continue to reward the high performers and the low performers. He is trying to get at very important objectives. I am not protesting that at all.

DR. NIEMEYER: I think you wouldn't do it that way. That's all.

DR. GOTTS: If I hear this correctly, this is what Skinner means by superstitious behavior. And the suggestion that we ought not to build superstitious behavior in which there is not a causal relation between what the child has done and what follows.

DR. ZIGLER: That's another aspect of this thing. If you were to force the work the way the child is supposed to do it, he is going to do what he was doing on that day when he got the reward. That would be a horrible thing.

DR. GOTTS: That's another argument.

DR. ZIGLER: You must realize, though, that in this particular experiment, you are giving a kid something, and one of the things that could happen is that you may not lower discrepancy. You may increase discrepancy, because the parents, whose kid is coming home all the time, may have made a perfectly good judgment. But by giving the child all these rewards, the parent might think, "Well, My Lord, I must have been underevaluating this kid." And you are going to increase the discrepancy.

MR. MANDEL: That is one of my hypotheses.

DR. ZIGLER: What I would like to see your research directed at is less concern about discrepancy and more concern about the parent's attitude towards the school and towards their child. The fact of the matter is, that many of these parents find that the only thing their kids bring home from school is "bad news." "Come and see me. Your kid's in trouble." It doesn't bother me if I have some independent evidence that these parents would like to receive this stuff, and can receive it. But I like the Paradise Room concept of tokens. If we could change these parent's attitudes towards the school with this kind of maneuver, well....

DR. COOKE: This is the ideal attitude of the parent, realistic optimism. I mean, this is the thing you would like to have each parent get. Yet you don't want him to be exactly on the button with every child. The stupid child, you don't want the parent to think he is stupid because he is going to keep him stupid. You don't want that.

MR. MANDEL: That's right.

DR. COOKE: You want a realistic optimism running through the attitude of each parent. To me, that's much more important than discrepancies. If you could define what realistic optimism is, that is thinking the child is a shade better than he actually is, then you would have parents working towards that child obtaining his realistic ability.

MR. MANDEL: That's right.

DR. ZIGLER: But, you see, you could very well manipulate the parent into having an unrealistic optimism about the child.

DR. COOKE: Right.

DR. ZIGLER: --and then your manipulation would hurt the child in the long run, because what good is that going to do the child or the parent? He might be terribly disappointed in the child later on.

DR. COOKE: If you had a way--and there may be ways--if you had a way of establishing what some of our people call "maximum expectancy level" for a child. And then your reward for the child, everytime he comes up to that expectancy level, and that doesn't mean what his average functioning is. It's what the top might be. Then you've got something that the child is rewarded for realistically and the family can find out that this child is actually doing the best he can, which

is the goal every parent would like to have for his child. And they get some feedback from the school that he is doing the best that he can - not that he is stupid, or that he is a genius, or anything else, I think, that to me, is what has to be engendered.

MR. SUGARMAN: But part of that child doing the best he can may be to establish some sense of optimism on the part of the parents even though it's a false one to begin with.

DR. PIERCE-JONES: And then temper it.

DR. COOKE: But you do that by setting some levels for the realistic levels - you, yourself, with that particular child. And then you begin to reward him for coming up to those levels and the kid takes the reward home.

DR. PIERCE-JONES: May I cut in here and ask if Mr. Mandel has any data analyzed, and, if so, what he has found out?

MR. MANDEL: Well, taking into consideration all of the comments about the design, I did analyze some of the data that I have collected thus far on forty-seven of my subjects. I have completed the interviews and tests. In my two by two design, I got an overall, between groups, difference (which I am not really concerned with) of less than .10 level.

DR. OSBORN: That's your four groups?

MR. MANDEL: That's the four groups. I got a reward high discrepancy versus a reward low discrepancy level significance.

DR. ZIGLER: Wait a minute. Did you go on from that point then? That sounds like interaction. What was your interaction?

MR. MANDEL: Interaction was nil.

DR. ZIGLER: So then you just picked two cells and did a t-test between them?

MR. MANDEL: Yes.

DR. GOTTS: Anytime, you have directional hypothesis before you begin in relation to all of your cells, you are perfectly justified in doing "t's".

DR. ZIGLER: That's right.

DR. GOTTS: An "F" is exploratory when you don't know what to expect.

DR. ZIGLER: That's true.

MR. MANDEL: I did have a directional hypothesis.

DR. ZIGLER: You have a prediction about no food reward in high discrepancy and no food reward in low discrepancy? Was that your prediction? I think your prediction is interaction not the difference between those two cells.

MR. MANDEL: Initially, I predicted an interaction. But I predicted that differently, those two cells in that way--in that direction. My cell mean for the rewarded high discrepancy group was 7.67. My cell mean for the rewarded low discrepancy group was 11.89. These are mean discrepancy scores - that's the change in discrepancy score between the initial pre and post measures.

DR. OSBORN: Would you give those again? I caught up with you now. I am with you up to there - for the reward scores.

MR. MANDEL: For the initial high discrepancy group who were rewarded, their cell mean was 7.67.

DR. ZIGLER: This was food reward high discrepancy?

MR. MANDEL: All right. That's an N of nine.

DR. ZIGLER: Okay.

MR. MANDEL: Food reward low discrepancy, with an N of 9, is 11.89. That's significant beyond the .05 level.

DR. ZIGLER: What does that show? Say it to us in words.

MR. MANDEL: All right. Not taking into consideration the alternative predictions brought out, to me that says that discrepancy change is a function of reward without taking into consideration the direction of the discrepancy of either the parent or the child. An overall discrepancy is a function of reward, simply because there was no significant difference between the "high discrepancy no reward" condition and the "low discrepancy no reward" condition.

DR. OSBORN: There was no significant difference?

MR. MANDEL: No significant difference. In fact, the cell means are very close together. They are 11.71 for the "no reward high discrepancy" group and 12.46 for the "low discrepancy no reward" group, with an "N" of 14 in the former cell and an "N" of 15 in the latter cell.

DR. OSBORN: Excuse me. If I understand your first two figures though, this suggests that the group which was more realistic in the beginning were also more realistic at the end. But the group that had the High "D" got better. Am I interpreting that correctly?

DR. ZIGLER: Well, this is 7.67 what?

MR. MANDEL: That's reduction in discrepancy.

DR. ZIGLER: So that's reduction in discrepancy?

MR. MANDEL: Yes.

DR. PIERCE-JONES: So that is change in difference scores?

DR. OSBORN: They both improved. But actually your better group got still even better?

DR. COOKE: The first one that you gave showed the most change. Is that right?

MR. MANDEL: The most change? Yes.

DR. COOKE: And it was the high discrepancy that was rewarded by food.

DR. PIERCE-JONES: That's right.

DR. COOKE: So food tended to reduce the discrepancy?

MR. MANDEL: Right.

DR. OSBORN: That's right. But I thought the group that was most realistic to begin with was the low discrepancy group. Is that right?

DR. GOTTS: This is the magnitude of discrepancy he wants, which means that the high discrepancy reward group has the smallest discrepancy score.

DR. OSBORN: I see. I see.

MR. MANDEL: Now, for the "no reward high" versus "no reward low," it was not significant. The "F" was .19, as a matter of fact. For the interaction of these cells, the "F" was .78, which was not significant. And I got a P of .10 between the rewarded versus the no reward group.

DR. ZIGLER: But that was all due to your food reward. I mean if you did a Duncan, you've got one cell that is giving you that.

DR. PIERCE-JONES: That's right.

MR. MANDEL: Right. Yes, the high discrepancy group.

DR. ZIGLER: You don't, then, have any general reward effect?

DISCUSSION

John Pierce-Jones and Committee

DR. PIERCE-JONES: I think Mr. Sugarman has one question to ask. Then I am going to break up our proceedings for the afternoon. You've had a long session without a break.

DR. ZIGLER: Yes. How come we didn't have a break?

DR. PIERCE-JONES: We didn't program a break. You still haven't heard the whole story, Ed, but we are not able to give you any more of it today. Jule?

MR. SUGARMAN: Let me make one announcement. On the Austin application, the examination of the application is completed. There are no problems in the examination of the Head Start portion of it. There may or may not be a problem in the Community Action Agency. Assuming that that may get resolved, there will be no problem with the program for the same number of children as are presently involved.

DR. PIERCE-JONES: So we may still have the opportunity for our work here?

MR. SUGARMAN: You are still probably going to be in business. Even if there should continue to be a problem for you with Community Action. Who is the delegate agency by the way?

DR. PIERCE-JONES: Formerly, it was the Community Council of Austin and Travis County. Now, the Community Council has given up sponsorship, and an independent, nonprofit corporation has been formed. It's called the Human Opportunities Committee.

MR. SUGARMAN: That's the Community Action Agency?

DR. PIERCE-JONES: I believe so.

MR. SUGARMAN: Do they operate the program directly?

DR. BOGER: Actually, I think it is the Austin Public School System.

MR. SUGARMAN: If there should be a problem with the Community Action Agency, it may still be possible to work out something with the schools.

Two points: first, is there any process for doing just the kind of critique that was going on here today to satisfy yourselves that you have a good setup?

Secondly, what about examination?

DR. PIERCE-JONES: Let me speak briefly to those two points. When I turned in our proposal last April, I had a provision in it for a panel of critics to work with us. As we finally negotiated the contract with IED, we cut back forty-five thousand dollars from the original budget. I felt that what could be sacrificed most readily were the honoraria and travel moneys for a critic panel. I was sorry to do it, but that's the decision I made at the time.

MR. SUGARMAN: So, at the moment, things will simply be completed, submitted, and that's it?

DR. PIERCE-JONES: Yes. Of course, our work will be submitted for publication, submitted to IED, and turned back to you. And there will, undoubtedly, be publication lags.

I am personally very grateful to have had you here. I think that my friend, Ed Zigler, has lived up to his usual self and put his finger on all sorts of critical things, for which I am very grateful.

DR. ZIGLER: I have a hunch that you've got plenty of skills in your group. I have heard some very sophisticated thinking here today. I don't think you need a bunch of experts to come here and tell you your business. I think there is plenty of ability right here in this room. All you probably need is some formalization of things, especially in these individual studies; you need to get together and criticize one another, and bring in a couple of other people from some other universities - people who haven't been working on your projects.

DR. PIERCE-JONES: Well, in frank defense of not having done that, let me point out that, when we got our contract from IED, it was written on a ten months basis - a final report was to be submitted on the 30th of June. In order to get the whole thing going, I decided we would simply have to design our studies as best we could so that we could have them done and reported by the contract termination date.

Subsequently, funding was extended through August. However, this won't help us a great deal at this juncture. I would urge,

therefore, that if the E and R Centers are going to be continued, and I think personally that they should be, then we need better financial assurances. Now that we've gotten a start, we can all be a good deal more productive, especially as time goes on. If we had at least the moral assurance that we were not involved in a one year shot--I realize there can't be a legal assurance, but we need a moral assurance - then it would enable us to lay our plans more strategically than we have been able to do this year.

MR. SUGARMAN: Let me say, as I said a little bit earlier to you, that I think OEO is committed to the continuance of the Evaluation and Research Centers. I think, however, that we will have to face two kinds of questions on refunding.

First: should we continue to use the mechanism of IED or fund directly? Second is the question of whether, based on what an institution has been able to accomplish thus far, we have confidence that they can continue to be productive.

I will also look very closely at whether there is a possibility of planning funding beyond one year. Legally, we can go to two years, but the problem is a dollar problem, that is, do we have enough money to fund for two years? We will see if there is any possibility of funding on a long-term basis.

DR. PIERCE-JONES: This would be most helpful. We have all felt an extreme pressure to be productive, to get studies moving. We might have benefited by a little more reflection time.

DR. OSBORN: I think it sounds very good, particularly when you think of one year, knowing you have to open the box, examine it, and close it up in that time. It hardly gives you the gestation time you would ordinarily appreciate. But to me, it does sound like a well thought-out program. I do want to say, for the Planning Committee (since most of them are university-raised) that they are very sympathetic to your problem. Knowing staffs, we realize that they are very inclined to go off to Indiana or someplace else. It's something that is forever being worked at with the government. And, of course, as Jule pointed out, the government has its own problems, too. And it is not that we are not sympathetic - for, heaven knows, nobody likes to operate on a hand-to-mouth existence. It makes it hard for a staff.

DR. PIERCE-JONES: Let me emphasize that in terms of this gentleman (Dr. Boger) right here. Are you still going to Michigan State?

DR. BOGER: Yes

DR. PIERCE-JONES: Was I able to assure you before last September first that you had a job for this year?

DR. BOGER: No.

DR. PIERCE-JONES: I don't think I could. And just because he was "guttzy" enough not to go to UCLA or North Carolina last year, and because the contract did come through, I was able to keep him here for a year. It's as simple as that.

DR. OSBORN: We are aware of it.

DR. COOKE: I'd like to express the thanks of the Committee for all the work that has gone into the session today. I think it's very valuable from our point of view. I am troubled that we are really expecting "instant research".

DR. PIERCE-JONES: You can't get it. At least not of high quality. You just can't get it.

DR. COOKE: Right. I am troubled that other government agencies can, in a sense, make moral commitments which they really have to honor for as long as six or seven years, while we are on a ten month basis. It just doesn't seem to me--particularly because the kind of studies that are outlined, the ones that I would give A plusses to - would be the ones that involve finding out what happened several years later.

DR. PIERCE-JONES: Yes.

DR. ZIGLER: I don't think there could be any question on this one, Jule. It takes longitudinal work and you have to go off in several directions. And that takes more than a group of people that you can get together for a year.

DR. PIERCE-JONES: That's right, Ed.

DR. ZIGLER: You have to have long term commitments by people whose research enterprise is nailed down in your organization and you go with them.

DR. PIERCE-JONES: That's right.

DR. ZIGLER: And that's the way it has to be done.

DR. PIERCE-JONES: Dr. Cunningham and Mrs. Coody can correct me on this one, but I believe we had to carry some of our staff on another project for a couple of months, didn't we?

DR. CUNNINGHAM: At the beginning, yes. No CPA would have done that. (Laughter)

DR. PIERCE-JONES: Of course, we used them for other work, too.

DR. OSBORN: This is one problem that research always has - it doesn't pay for itself, and a university is stuck with a permanent staff within projects. If you don't get stuck with them, they leave. Research is a very costly endeavor. If you are going to keep a permanent staff, you've got to pay them between projects. Nobody else is going to do it.

DR. ZIGLER: Of course, there is another part of it. Although we can't get involved in the business of the University of Texas, as a policy matter, we would like to see universities commit themselves in the way the Dean said he was committed. In other words, we should look with great favor on those universities which create permanent research openings for our enterprise and for the people we select to run them. I don't see that it would hurt somebody like Pierce-Jones (and the other directors) if we were fairly frank about this. Certainly, when an NIMH or NICHD grant application is evaluated, especially a longitudinal one, we are always concerned about the commitment of the university? What is it really willing to commit? In other words part of the problem is a university problem, not just a money problem.

DR. PIERCE-JONES: It certainly is!

DR. ZIGLER: I think we might be helpful simply by saying, just as would any other federal granting operation, "We are interested in the kind of commitment and the kind of support you can count on from your university!"

DR. PIERCE-JONES: Ed, this could help tremendously. I have run into this problem, within the university, repeatedly. They have not, I believe, felt secure with OEO funding for the Center.

DR. OSBORN: This is a problem all universities face. Back to what I said earlier: they have to think about the time when the funds are gone and they are stuck with the staff.

DR. ZIGLER: Well, to pressure Holtzman, we are going to have to be in a position to say, "Look, we are going to give you the money."

What are you going to do with us?"

DR. PIERCE-JONES: Dean Holtzman has been willing to gamble a good deal with us in this. He has been very supportive. He's been extremely helpful.